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ABSTRACT

The papers included in this compilation were presented at the 1971 meeting of the Panel on Science and Technology of the U. S. House of Representatives Committee on Science and Astronautics, and concern various aspects of international science policy. The papers include an address by the Secretary of State on "U. S. Foreign Policy in a Technological Age," and reports by U. S. and foreign scientists and administrators. The reports concern international cooperation in the physical, social, life and environmental (including marine) sciences; administrative mechanisms for increasing cooperation, including the necessary prelude of a national science policy; the role of legislation in advancing cooperation; the role of a science policy in solving social problems, military and arms control, and the potential consequences of experimentation with human eggs. Although all papers have international implications, one deals specifically with science, technology and the developing countries. (AL)

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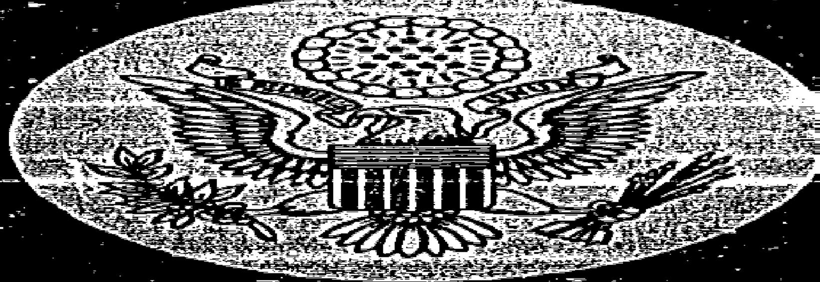

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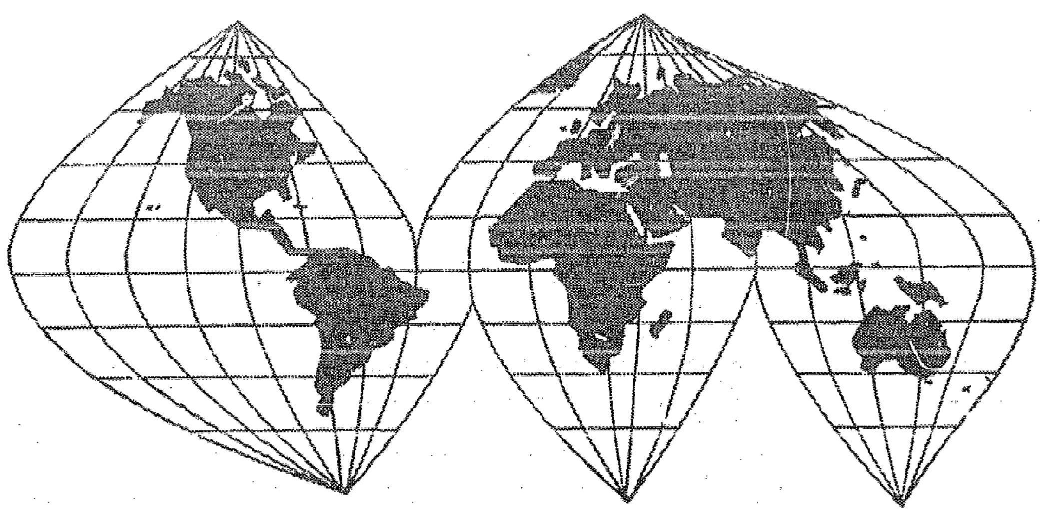

A Compilation of Papers Prepared for the 12th Meeting of the Panel on Science and Technology 1971

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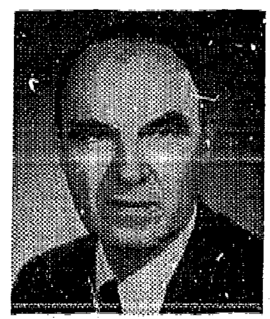

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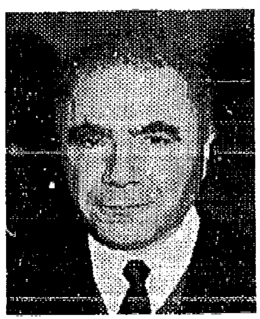

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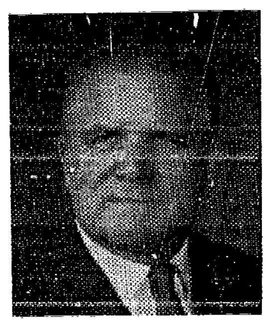

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INTRODUCTION

How can science and technology be utilized in the solution of today's critical national and international problems? This question was examined by perceptive scientists, educators, and legislators at this year's meeting of the Panel on Science and Technology with the Committee on Science and Astronautics of the House of Representatives.

More than ever before science is international in scope and character. That is why the Committee chose "International Science Policy" as the theme of our 12th annual Panel. Participation was indeed international. Ideas and information were exchanged by invited speakers from 7 foreign countries on 4 continents, joined by a like number from the United States.

The papers which they presented are learned and provocative. They deserve wide distribution. Accordingly, they have been compiled in this limited document which serves as a special supplement to the complete Proceedings of the Panel already issued.

The Compilation of Papers contained herein is offered to the Congress and to all people in the conviction that intelligent application of science and technology, coordinated on an international scale, is essential in order to cope with the social and political challenges facing mankind today.

GEORGE P. MILLER,
Chairman.

FEBRUARY 1971.

INTERNATIONAL SCIENCE POLICY

KEYNOTE ADDRESS

U.S. FOREIGN POLICY IN A TECHNOLOGICAL AGE

WILLIAM P. ROGERS [1]

For more than a decade now this committee has performed an important service by bringing together every year some of the best scientific minds of the world to discuss problems of great relevance to the future of mankind. Science and technology have come to play a role in international relationships far beyond what any of us would have anticipated even a generation ago. In our foreign policy we must take account of this fact.

In so doing, we build on a long-established tradition. From its earliest days American foreign policy has encouraged the international spread and application of scientific knowledge.

During the Revolutionary War, Benjamin Franklin ordered U.S. warships not to impede the explorations of Captain Cook, although he was sailing under an English flag.

In 1853 the oceanographic research of an American naval officer, Matthew Maury, brought the maritime nations of the world to Brussels for a conference which marked the beginning of international cooperation in navigation and meteorology.

In 1949, President Truman in the point four program pledged to make American scientific advances available for the growth of less developed countries—a pledge which every President since has renewed.

In today's world of dramatic technological progress the imperatives which created the three historical episodes I have cited have not vanished.

It is as necessary now as it was in Franklin's time to insulate activities which promote scientific progress from the vagaries of international policies.

States will still benefit from cooperation in the use of the world's oceans, just as they will benefit from sharing the knowledge which modern satellite technology can bring.

The need remains as great as it was when Point Four was proclaimed to apply technology to the problems of development. Indeed, popula-

[1] Secretary of State, United States of America.

tion pressure in some nations is so acute that even the greatest agricultural breakthrough in decades—the development of the grain which created the green revolution—has only, in the words of the American agronomist who won the Nobel Prize for it, "bought time."

This administration is adapting American foreign policy to the fact that never before have the global characteristics of science and technology held so many consequences for so many people.

We in government understand that if technology has given man the power to destroy, it has also given him the means to curb that destruction. The limited nuclear test ban treaty and the non-proliferation treaty would be less valuable without the means which technology has provided to detect violations. If the United States and the Soviet Union reach a strategic arms limitation agreement, it will be partly because technology has made possible reliable verification techniques.

National policy can determine whether the use to which technology will be put is to be good or bad. Policy choices are crucial in deciding whether the atmosphere will be cleaned up or poisoned further or whether the seabeds will be exploited for the benefit of the exploiting nation or for mankind in general. As the principal creator and the principal user of the new technology, the United States, in its Government's policies, is taking such questions seriously and acting on them.

This country is the biggest consumer of the world's energy and resources. With only 6 percent of the world's population, we use 40 percent of its energy output and 40 percent of its nonrenewable resources. While estimates vary, there are some who believe that the earth's petroleum supply can serve as a major source of industrial energy for only another century. However accurate this prognosis turns out to be, there is no doubt that such resources are finite. The metal production of the globe after World War II is already as great as total world metal production before it. And we, like all other industrial nations but the Soviet Union, import most of the minerals and ores we use. These facts, well known to most of you, illustrate the degree of international interdependence to which we are subject.

In short, in the area of science and technology we are operating in an international environment, both because we want to and because we have to. This fact has given our foreign policy in recent years a whole new dimension. It is symbolized by the presence here today of the science attachés in a number of embassies in Washington and also of our own science attachés stationed in 17 countries around the world. Indeed, it is to be expected that at a meeting such as this, the U.S. Congress would seek the views of such leaders in science as those with us today from North America, Western Europe, Kenya, Pakistan, and the Soviet Union.

Our basic goal is to put science and technology at the servic, of human—and humane—ends.

In our foreign policy we are taking three major steps to achieve this goal:

1. We are increasing our emphasis on science and technology in our aid to developing countries.

2. We are encouraging an international effort to preserve the quality of the world's environment, and

3. We are seeking greater international cooperation to enhance the benefits of technology and to curb its dangers.

Let me discuss these three areas separately.

President Nixon, in his foreign policy message to Congress last February, said: "Unprecedented scientific and technological advances as well as explosions in population, communications and knowledge require new forms of international cooperation." In his September 15 aid message to Congress the President sought to put this concept into action by proposing the creating of a U.S. international development institute "to bring the genius of U.S. science and technology to bear on the problems of development."

Such an institute could help developing countries gain the technical capabilities to solve their local problems. It could make possible broader technical cooperation with countries which have ceased to receive, or have never received, aid funds. And it could put to increased use research of the sort which made the green revolution. This new approach would require greater help from the scientific community than we have ever sought before.

Satellite technology holds great potential for development. Already international cooperation in the U.N. framework is well advanced in weather forecasting. The day may be near when weather can be predicted weeks in advance. This would be an obvious boom to countries dependent on agriculture. Indeed it may become possible to modify the weather on a significant scale, increasing rainfall and decreasing the intensity of certain types of storms.

With such improved techniques of weather management it may be possible to reduce the severity of catastrophies of the sort that befell East Pakistan.

We would like to apply this as it becomes operational to the problems of developing countries. We are also alert to the need to consider international arrangement to deal with the implications of this new phenomenon.

The experimental earth resource satellite program, pledged to international use by President Nixon at the U.N. in 1969, is also rich in development possibilities. These satellites should have the potential, among other things, to monitor water resources, measure the extent of snow coverage, determine timber growth, and locate crop diseases

and schools of fish. We have invited other countries to propose experiments in this program. And we have begun training programs in other countries, including Mexico and Brazil, to provide them the skills needed to use the data obtained from remote sensing. There will soon be a need for an overall international framework within which this knowledge may be accumulated, analyzed, and shared.

In another use of satellite technology, India and the United States are currently engaged in a joint experimental effort to bring educational television to Indian villages by satellite.

Technology is rapidly giving man the ability to exploit the resources of the deep seabeds. A proposal made by President Nixon last May seeks to make those resources the common heritage of mankind. In a pioneering approach it provides for an international regime to regulate exploitation beyond the depth of 200 meters. It also provides for a system of royalties a substantial part of which would be used for economic assistance to developing countries. One day this approach may prove valuable elsewhere.

This administration also favors an expanded program of technical assistance within the International Atomic Energy Agency so that the benefits of nuclear technology in medicine, agriculture, hydrology, and industry can be made available to the developing world. The Agency's essential task of providing safeguards against the diversion of nuclear material for weapons purposes should not obscure its important development function.

The U.S. Government is very conscious of the opportunities for economic development that technology is providing. We accept the obligation to do all we can to help the less developed countries meet their development through more effective use of technology.

Turning now to the second area I mentioned, the problem of safeguarding the thin envelope of earth, water, and atmosphere which supports the only life known anywhere is becoming increasingly a global concern. In our foreign policy we have made protection of the human environment a matter of special emphasis.

At the last session of the United Nations General Assembly, the United States urged that the U.N. develop the capacity to keep track of pollution dangers and to explore the possibility of establishing international air and water quality standards by concept that the U.N. would identify on a scientific basis those pollutants that may be dangerous on a world scale and make plans for a coordinated worldwide monitoring network.

This approach, in assigning a major role to the U.N. recognizes the need for greater involvement of the international community in pollution control and for greater coordination in international efforts to protect the environment.

The United States has played a leading role in stimulating international bodies like the U.N.'s Economic Commission for Europe and the OECD to apply themselves to problems of the environment.

It was at President Nixon's suggestion in April 1969, that NATO established its Committee on the Challenges of Modern Society to deal largely with environmental issues. We have urged these organizations to emphasize action in their programs. And, we have been successful in encouraging them to avoid duplication, with each making the most of its particular capacities.

Just as President Nixon, in his first official act of this decade signed into law the National Environmental Policy Act, we would like to see the adoption of an international environmental policy. We would like to see also international lending institutions, in projects they support, encourage adequate concern for environmental implications.

Even beyond this, perhaps it is time for the international community to begin moving toward a consensus that nations have a right to be consulted before actions are taken which could affect their environment or the international environment at large. This implies, of course, that nations contemplating such actions would be expected to consult in advance other states which could be affected.

Pollution has mistakenly been called a "rich man's issue." But it carries with it dangers for the developing countries as well. Those countries can avoid many of the mistakes that the developed world has made in mismanaging natural resources. It would be tragic if Lake Tanganyika went the way of Lake Erie.

This administrtion is convinced that environmental improvements can—and should—be related to economic development in a way that enhances rather than retards it.

These issues are among the basic challenges facing the international community. They will be major tasks for the U.N. Conference on the Human Environment, which willl meet in Stockholm in 1972. We are determined to do all we can to make that conference a landmark in achievement. In this connection I am pleased to announce that we are forming a citizens' advisory committee to bring to our preparations for the conference the views and support of interested America groups and individuals.

As I have stated, it is our firm desire to ensure that the opportunities opened up by technology are explored in a spirit of international cooperation.

We are convinced that it is in everyone's interest, including our own, to do everything we can to facilitate the widest possible spread and exchange of scientific information. Thus, it is the general policy of this administration to permit the exchange of unclassified scientific and

technical information with the scientists and institutions of any country, regardless of the state of our diplomatic relations with that country.

To further facilitate the exchange of scientific information—and scientific cooperation in general—we are increasing our bilateral cooperative agreements. Before the 1960's the United States had no general bilateral scientific agreements with any country. Today there are agreements with ten countries, including most recently an agreement with Spain incorporated in the general agreement of friendship and cooperation signed last summer. These agreements cover subjects ranging from medicine, forestry and fundamental science to more recent concerns like pollution, noise abatement, and drugs. Several more agreements are under negotiation.

Scientific and technological cooperation is not a one-way street. We have as much to gain as anybody from cooperation. We can profit, for example, from Europe's experience in dealing with problems like land use and urban planning. We expect to learn much from our participation in a French experiment to build a pollution-free city in northwestern France.

Even in areas where our scientific expertise is paramount, cooperation gives us tangible advantages.

Let me cite one striking example out of many: Latin American and Caribbean countries, with annual assistance from our National Weather Service of about $375,000 play an important role in weather forecasting, with benefits for them and for ourselves as well. In 1919, well before this program was instituted. Corpus Christi, Tex., was devastated by a hurricane and 284 persons were killed. In 1970, Hurricane Celia, of about equal force, struck the same city but killed only 13 people, although Corpus Christi's population had multiplied 20 times. The early warning made possible by this cooperative weather program allowed the evacuation of 30,000 people and by conservative estimate saved at least 2,000 lives.

Even our Apollo program has leaned heavily on the cooperation of other countries. One of the first activities the Apollo 11 astronauts carried out on the moon was to set up equipment developed in Switzerland for measuring the composition of the solar wind. The Apollo program could not have succeeded without worldwide tracking stations, which had to be established by international agreements. It also needed emergency landing arrangements, which we negotiated with 80 countries. And we shall not soon forget the offers of assistance during the return to earth of the crippled Apollo 13 spacecraft and the voluntary radio silence on certain frequencies to facilitate communication between the astronauts and the earth.

We now want to go further and make the exploration of space a truly international endeavor. Space should not be the exclusive pre-

serve of a small number of countries. Our post-Apollo program aims at the development on a multilateral basis of a new generation of reusable space vehicles designed to make the exploration and use of space easier, and more economical. We are currently engaged in international consultations to bring this cooperative effort to reality.

As I am sure most of you are aware, Dr. Low of NASA has just returned from a discussion in Moscow of further cooperation in space activities with the president of the Academy of Sciences of the Soviet Union. I understand that they reached an agreement, subject to confirmation by governments. It covers such subjects as an early exchange of lunar samples, satellite meteorology, study of the natural environment, space research, and space medicine.

The current situation with respect to enriched uranium fuel offers significant opportunities for furthering international cooperation in the peaceful uses of nuclear energy. Because of its advanced technology and plant capacity the United States has been in effect the only exporter of enriched uranium fuel for power reactors. Indeed our export earnings from sales of nuclear powerplants, fuels, and related services, are over $1 billion now and are expected to reach $5 billion by 1975.

But worldwide demand for this fuel continues to grow, and it is clear that other nations intend to diversify their sources of supply. We are now considering whether, with adequate safeguards, we should offer to share our uranium enrichment technology with other nations building a civil nuclear industry.

Because the problems dealt with by science usually have a low specific gravity in political terms scientific cooperation is often possible where political cooperation is not.

This habit of cooperation is a good one to keep. If it is kept, it will surely have spillover effect in increasing the constructive role of international organizations, in establishing new patterns of international cooperation, and in strengthening observance of international law.

We in this Administration are fully aware of this potential to which I referred. We believe that this kind of international cooperation will do a great deal to assist in the achievement of our foreign policy objectives which are peace in the world and friendly relations among nations. It is in this spirit, Mr. Chairman and ladies and gentlemen, that I welcome you here in Washington on behalf of the President of the United States, and wish you great success in your endeavors.

INTERNATIONAL SCIENCE POLICY

REMARKS OF THE MODERATOR

ADRIANO BUZZATI-TRAVERSO [1]

I wish to bring to your attention some thoughts which I think resulted from the words we heard this morning and also, of course, from thoughts that I had in the past as a scientist before, and as an international civil servant now—a civil servant whose duty it is to foster scientific cooperation throughout the world.

Secretary Rogers spoke as follows: "It is as necessary now as it was in Franklin's time to insulate activities which promote scientific progress from the vagaries of international politics."

It seems to me that this sentence is the core of any international science policy. I will not spend any time on the international nature of the scientific endeavor, but I wish to draw your attention to the fact that to some extent throughout the history of science, but primarily in the course of the last 25 years, after the end of World War II, the ideal of international science has been limited by the fact that the scientist has become at the same time a servant of the state, a servant of the government. Unfortunately, the ideals of complete international cooperation have been and are yet sometimes incompatible with the necessities of the state, of the government.

For this reason, we find ourselves today in a strange position as scientists. Sometimes our loyalties are mixed and incompatibilities arise between what we think is our duty as scientists to work for truth, disregarding national boundaries, and the need we have to rely on the government to give us money for research while at the same time the government has to rely on us for our advice and our work for the aims of the government.

I will not elaborate on this because these, I am sure, are sentiments which all of you share. For this reason I think that it might be proper to consider a new mechanism for scientific international cooperation, a mechanism which is far away in the future but which perhaps would offer a solution to the present dilemma. This would consist in the abolishment of national research councils and similar bodies and in the establishment of one world research council which would be in charge of the development of science throughout the world. This, of course, would be the preliminary step toward world government.

[1] Assistant director general for science, United Nations Educational, Scientific and Cultural Organization (UNESCO), Paris, France.

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And on an occasion like this, a meeting of this type would not be held in the House of Representatives of this great Nation but it would be held in the House of Representatives of a world government. I realize it is a faraway possibility, but since the scientific community has been working in the past as much as possible to foster contacts between scientists of all nations, similarly I think that for the future, this might be a task toward which it might be worth working for.

INTERNATIONAL COOPERATION IN THE ENVIRONMENTAL SCIENCES

WALTER ORR ROBERTS [1]

I. THE NATURE OF THE ENVIRONMENTAL CRISIS

Two striking realities of our present age focus the environmental crisis of modern man. *First*, it has at last become clear that mankind has assayed what are probably the bulk of the exploitable natural resources of the planet, and there is no prospect, within these bounds, that we shall be able to give all the people of earth access to the commodities and services now available to persons of average or even below-average income in the rich countries today. It would take more than a hundred-fold expansion of the rates of extraction, for example, of iron, copper, and other metals, even if we used great ingenuity in the use of synthetics.

Second, the adverse side-effects and the sheer volume of wastes of the many products and processes of advanced technology threaten disruption of the stability and recuperative qualities of the natural environment of man. These detrimental effects on land, air and water, both present and potential, imperil the health and welfare of all living species, and dim the prospects for achieving, for ever-increasing numbers of people, the amenities of the good life that we have always assumed were inherent in social and economic progress.

This, then, is the nature of the environmental crisis. Countermeasures to avert the consequences of the crisis, as McGeorge Bundy stated here last year, appear to have approached the top of mankind's agenda. According to a recent Harris poll, Americans now regard pollution as "the most serious" problem confronting their communities. If this broad concern does not degenerate into a widespread despair over the possibility of environmental repair, the public desire for cleaner air and water, for quieter cities, for access to nature in an unspoiled state can lead to significant positive results. If despair prevails, however, it may well be that the best days of the earth have come, and are passing. However, I am an optimist both in respect to what mankind can and will do.

My own profession is astronomy, and because of this I tend to think of the Earth, first, as a planet of a minor star among the billions of stars of the billions of galaxies in our universe. In this context, our

[1] President, University Corporation for Atmospheric Research, Boulder, Colorado.

cosmical home is a miraculous and beautiful abode for life. It is very probably an extraordinarily rare one, too. Our moderator, Thomas F. Malone, one of this nation's truly distinguished experts in environmental sciences, introduced the environmental crisis in this way:

> It is generally agreed that Spaceship Earth was launched about five billion years ago. Life first appeared on Planet Earth approximately three billion years ago. Evidence of human life goes back, say, three million years. Modern man emerged about fifty thousand years ago.
>
> Informed conjecture on the solar energy still available and on the probability of cosmic accidents encourages us to believe that it should be possible—barring man-made catastrophes—to sustain life on our planet for at least three million more years. If this desideratum is actually to be realized, it is clear that some planning may be required.
>
> These plans should take account of two phenomena that are beginning to pose serious problems of a global nature in this latter part of the twentieth century. A population growth of two to three percent and a real economic growth of, say, four percent per year, when cast against a physical environment of rapidly constrained dimensions and natural resources of absolutely limited quantity, are presenting mankind with . . . formidable . . . problems of a global nature."

It is heartening that the word "ecology" has taken on meaning throughout the nation, and indeed a good part of the world. For Joseph Conrad once asserted that one can determine the leading and guiding traits of a culture by a few single words to which it pays respect.

In a fascinating essay on what he deems to be the coming struggle for amenity rights, Edward J. Mishan says that the keyword to which much of the world paid tribute between World War I and World War II was "thrift," but that since then the magic words are "challenge" and "growth." And he points out that challenge and growth, coupled together, make a two-headed penny. "Growth" sanctifies the heedless forward thrust of technology, and "challenge" pulls the rug out from under those who would move slower, or develop alternative commitments.

The challenge of space, and the challenge of travel faster-than-sound come at once to mind. But a reaction is brewing up, and it looks as though it may have staying power. There is a growing apprehension, throughout the world, that science and technology are dangerous tools, unstoppable and uncontrollable when driven too rapidly and heedlessly forward by a culture primarily worshipful of challenge and growth.

I suspect that in future perspective, the decade of the 70s will appear as the time when the word "ecology" attained the highest tribute, and replaced "growth" as a keyword for the age. I sincerely hope so. If it does, I am certain that science and technology, as tools shaped to serve the goals of man, can aid the achievement of the amenities of life throughout all the world. They are powerful means, and can be used in ways that will help bring man and the environment into the kind of stable, ecological balance that will give us a chance to sustain the good life, the age-old dream of philosophers, for at least a fair spell of Tom Malone's "three million more years" of our planet and to extend its blessings to the preponderance of human kind.

The cry for concern with ecology is not likely to be a short-lived alarm. It has survived prophets of doom who have gained attention by exaggerating the pace at which pollution is engulfing the biosphere, and the immediacy of dire consequences. The realities hold sufficient threat to spur urgent world-scale action.

In every part of the world thoughtful men ponder the worsening plight of rivers and lakes, of the air, and of the land. Human wastes dumped from inadequate sewage plants flood the streams of many regions of the earth. Industry, in many nations, pollutes the rivers and lakes with foreign wastes such as detergents, heat, organic chemicals, and toxic substances such as mercury compounds which have now been found even in ocean fish. Agriculture does its insidious harm through excess fertilizers and insecticides leached from growing fields, and through animal wastes dumped from feeding lots. As the per-person-pollution mounts, the population boom adds fuel to the fire.

These insults to natural water bodies degrade their available oxygen supplies, alter their ecological balances, and in extreme cases, produce eutrophication that imperils the very continuation of desirable life in these waters! Lake Erie, joint asset of the U.S. and Canada, is one of the worst-polluted major lakes of the world, a nearly-dead sea. Even that pride of Soviet lakes, Baikal, abode of rare and extraordinary wild species, is showing signs of encroaching contamination.

Air pollution, here and abroad, is an equally pressing problem, as crisis "smog alerts" threaten more cities ever more frequently. To quote John T. Middleton, who heads the National Air Pollution Control Administration:

> Our air is heavily burdened with dirt and chemicals. It constricts our throats and makes us sick; it even kills some of us ahead of our time. It besmirches our great cities and destroys some of the material goods and pleasures for which we began enduring all of the pollution in the first place.
>
> We could solve the problem instantly by stopping all our cars and closing all our industries . . . . and in time the air would purge itself of most of its impurities. . . .

But, of course, this is not a solution, and Middleton does not pose it as such. Instead, he proposes solutions through regulation of effluents by law and compact, and through institution of financial incentives to find new ways to do the jobs with less damage. The automobile is a prime offender. It produces more than half of our nation's hydrocarbon pollution, nearly half the nitrogen oxides, two-thirds of the carbon monoxide and most of the lead. The solution will not be simply to ban the car, but rather to alter its design, its fuels, and its patterns of usage to control its adverse environmental impact. In areas of the world that have not yet made our automotive mistakes, our improvements can be their starting points, to the benefit of all.

Environmental pollution problems are deeply entwined with the whole fabric of national and world society, and will be costly to repair. As McGeorge Bundy stated last year (2), there will be no obvious, consensual and painless technical panacea available to us to solve our environmental problems. There will be sharp political conflict over the assignment of additional cost burdens involved in correctives. It will be necessary, I suspect, for us eventually to become aware that in paying greater costs today we are assuming, as we must, the costs left us as a heritage from the mistakes of an earlier generation. It is possible that we may soon be up against unquestionable evidence, as Bundy also observed, that further environmental recklessness will lead to irreversible damage. As he said, "The values of our society and the quality of our politics will surely be tested sharply by these choices between adequate and insufficient action; by the assignment of the burden between producer and consumer, between private and public sectors, and between present and future generations." And Bundy could have added that the assignment of the burden between the developing and the developed nations will impose new and severe strains on international mechanisms. These strains will, I believe, necessitate institutional innovations of international character. I will say some words later about this.

Another of last year's panelists remarked that perhaps it was easier to go to the moon than to face up to what is happening in the street outside. I believe only that it is more difficult; I do not think we will fail to do it, for the stakes are vastly greater. I believe that the needed technological tools and world will are at hand, or very nearly so. The age of ecology may be the age in which men of the earth learn to live abundantly and in balance with the finite resources of the biosphere. I have a further hope that through the cooperative actions needed for saving the environment, the peoples of earth will also learn to achieve a political harmony that echoes the coexistence of diverse natural organisms in a bounded ecosystem. If so, the age of ecology will indeed be a new and better age for mankind.

II. Two Pivotal International Environmental Programs

The problems of environmental pollution are enormously complex. It would be a grave error to think that we fully understand all we need to know in order to control environmental degradation. But much *is* known. We have confidence that we know the necessary directions to which to vector top priority attention on many key environmental questions today. These matters have been widely debated in numerous national and international scientific-technical study programs and seminars.

In this section of my report, I single out two pivotal, high priority areas of international research and engineering effort that require international cooperation for their success, and that are important building blocks if we are to understand our natural environment and from this optimize our use of its benefits.

In a later section I still speak more briefly of other important international environmental science programs that have valuable contributions to make.

A. The Global Atmospheric Research Program

The Global Atmospheric Research Program is probably the most ambitious international undertaking ever conceived in the environmental sciences. Its goals go far beyond environmental conservation, and encompass a new and ambitious world-cooperative attack upon the fundamental unknowns governing large-scale weather and climate phenomena. Its benefits encompass an improved understanding of the maximum lead-time to which forecasting of the detailed circulation of the major wind and weather systems can be extended.

The Global Atmospheric Research Program, usually referred to by its not-so-euphonious acronym "GARP," will obtain real-world weather observations of adequate day-to-day resolution *for the whole globe*, for a test period. At present, the world-cooperative weather networks bring only about one-fifth of this amount of information to research workers. These data will be used as an integral part of the GARP effort, to seek new levels of weather and climate forecast skill.

With real-world data of high resolution and known accuracy, teams of scientists working cooperatively in many countries will make experimental forecasts by various means and at various leadtimes, and will have objective data against which to judge their success and refine their theories.

It sounds simple, in principle, but it is a major task. It would go far beyond our interest today, if I were to detail for you the many steps involved in reaching the GARP goal. But let me outline a few salient features of the plan.

First, GARP came about because scientists in a number of countries believe that new tools, primarily computers and satellites, now give us a prospect of a truly new approach to understanding and predicting the atmosphere's main behavioral features. Among these tools were earth orbiting artificial satellites, and the giant computers now built or on the drawing boards. The forecast equations involved have long been known—in fact L. F. Richardson had explicitly formulated them in 1923. But computers potent enough to solve them are new.

Today, for example, Dr. Warren Washington, in the National Center for Atmospheric Research, using our CDC 6600 computer can solve the intricate meteorological equations for over 8000 measuring locations of the earth, at two atmospheric levels, at six minute time-steps, and do so in a matter of minutes for a day's synthetic forecast. I say "synthetic" because till now he must use, primarily, artificial (not real) weather observations. The computer he uses generates 38,000 numbers every five seconds of running time, and plots the results as weather maps. The technique is called numerical weather model experimentation. Real weather is being artificially simulated in a computer, when Washington carries out his modeling.

Other centers, such as the NOAA Geophysical Fluid Dynamics Laboratory at Princeton, have similar model experiment capabilities and programs.

Washington must have computers with a hundred-fold to a thousand-fold greater speed and capability to do his simulation with the realism we believe necessary to achieve our forecasting advances. He must have access to a computer that can add not three-million numbers per second, but something like a billion. Such super-computers are in prospect for the middle of this decade.

The aim of GARP is to supply scientists like Washington with real-life world-wide data to put in their simulation experiments, and against which to verify their success.

To care for and feed super-computers like these, and to generate the software to assure that the simulation goes efficiently, will take scores of people and years of effort. And the magnitude of the GARP task, moreover, demands that perhaps a minimum of a half-dozen such numerical modeling groups work in parallel on carefully apportioned tasks in the overall effort. Probably two or three such groups should be established in this country.

The numerical model is only a part of the story, albeit a crucial part.

The computer must have data taken at the right scale, and of the right things, with adequate accuracy. Atmospheric dust, for example, affects the heat balance of the atmosphere, so the computer model must have relevant measures, and know how, from its software, to use them. Ocean temperatures, earth topography, earth reflectively (albedo), including snow cover, forest vegetation, and other relevant

data must be included. And, above all, the entire earth must be encompassed with day-by-day point-by-point values of wind velocity, temperature and humidity, at every 500 km (300 miles) distance in latitude and longitude and for at least a half-dozen heights.

Obviously it will be a big step from today's population-oriented weather measurements to the GARP ideal. Vast fractions of today's weather trends are scarcely observed at all. But many nations are already committed to the goal of achieving and using such data. The prime goal is the "First GARP Global Experiment" in the latter half of the 1970s.

President John F. Kennedy, in a momentous speech before the United Nations General Assembly in 1961 set the wheels in motion by calling for "further cooperative efforts between all nations in weather prediction, and eventually in weather control." Two subsequent UN resolutions gave specific substance to a cooperative effort, which has culminated in the two-pronged World Weather Watch: (1) the evolving operationally-oriented world weather system, and (2) the research-oriented Global Atmospheric Research Program. My old and revered friend, the late Harry Wexler of the U.S. Weather Bureau, and my much newer friend, Dr. V. A. Bugaev of the Hydrometeorological Center of the USSR, coined the name World Weather Watch, and paved the way for the subsequent major world cooperative research thrust in GARP.

The 90th Congress of the U.S. endorsed our nation's participation, in May 1968, through Senate Concurrent Resolution 67.

Our moderator, Tom Malone, and the distinguished Swedish scientist, Dr. Bert Bolin, have given and still give outstanding leadership to the international effort, which culminated, some years ago, in an unprecedented agreement by a UN-related organization, the World Meteorological Organization, and a non-governmental organization, the International Council of Scientific Unions, jointly to plan a major world cooperative research program. A committee of 12 men from 10 countries directs the effort, with staff support from Geneva.

Robert M. White, the Administrator of NOAA, has given high priority to GARP in the U.S. World Weather Program. The National Academy of Sciences sponsors a 14-man "U.S. Committee for the Global Atmospheric Research Program," under Jule G. Charney, to oversee the U.S. effort, which will deeply involve universities, the government, and industry. Charney's own personal contributions have given us much of our confidence that the GARP goals are soundly conceived—but the planning effort has been a long and careful one, and many distinguished scientists have been involved.

Our own laboratory, the National Center for Atmospheric Research, is already deeply involved in various parts of the research, particularly in numerical model simulation. We have also taken on development

efforts for special balloons, aircraft, and other facilities needed in GARP.

GARP's first major international field experiment is the "GARP Tropical Atlantic Experiment" probably to be carried out in the summer of 1974. The effort will focus on the Equatorial Atlantic Ocean, and will involve special studies of tropical wind, moisture transport, cloud systems, turbulent flux of energy and other atmospheric processes. Tightly-coordinated measurements programs will involve more than ten different nations, who will contribute extra-territorial facilities, such as ships, ocean buoys, special balloons, instrumented aircraft, and satellite time. More than 30 other nations will make specially accelerated land-based measurements during the experiment.

The next major GARP effort will be the "First Global GARP Experiment," as I mentioned above. This is a truly global weather observing effort over a specific test period during which many specialized measurements will be made. It will be the first effort at obtaining truly global weather data coverage for use in large-scale weather simulation experiments. The data will be available to all nations.

Many other preliminary and essential cooperative steps are in progress. For example, about 35 scientists in some 13 countries are now at work on special numerical model efforts designed to assure that future models will be adequate to the job of efficient exploitation of the later special observing periods, when the data arrive.

The scientific and practical gains to be expected from this remarkable program are far reaching. From the effort we hope to make a significant advance in weather forecasting skill. The most reliable estimates suggest that large-scale atmospheric motions are potentially predictable to a limit of at least two weeks. Thus it seems theoretically likely that we will be able to make useful forecasts of daily sequences of large storm systems and other weather features out to a two-week lead-time, and perhaps longer. Statistical trends may be determinate, and capable of useful forecasting for longer lead-times.

But only a fully global scale effort like that projected for GARP can make the "great leap" that Jule Charney has identified as necessary to be sure that our theoretically-based expectations are truly realizable.

The potential benefits of forecast improvement need little explanation. They encompass agriculture, construction, electricity and gas power management, water conservation and distribution, transportation, recreation, and innumerable other uses. The benefits extend into every segment of our national and international activities.

To me, however, a prime benefit will be the confidence we will have if we can verify the validity of global numerical weather simulation models with real data. Armed, then, with credible models, I would hope that we could simulate vast numbers of highly important real-

world weather experiments economically and without the risk of gambling huge sums of incurring the adverse side effects involved when we experiment in nature. In the computer we could modify hurricanes, release huge dust clouds, spread artificial cirrus, eliminate smog, and do vast numbers of other experiments to assess their feasibility and their side effects, as a guide to understanding what may happen in real life. This benefit alone may justify the multi-million dollar world-cooperative effort, in my view.

Still another great benefit will be in the strength it can give to the sound cost-benefit assessment of given levels of improvement in global weather and pollution monitoring systems for future operational systems.

Finally, I regard GARP and the follow-on weather applications of space as one of the most promising and economically justifiable of all suggestions I have seen for the post-Apollo exploitation of our national capabilities in space.

In summary, I believe that the GARP program merits priority attention in national and international science and technology priorities for the following reasons:

1. Its underlying aim is the benefit of all mankind through improved knowledge of weather and climate.
2. It is made-to-order for international cooperation, and in fact cannot come about without extensive cooperation that will involve many of the developing countries in the use of available tools and techniques to give regional data for a global network.
3. It excites the interest of the general public as do few scientific or technological questions—especially since it includes work on many important aspects of weather and climate modification, both deliberate and inadvertent.
4. Space technologies are essential to its success, though it also makes strong demands upon conventional meteorology, a field with an already remarkable tradition of international cooperation, on which to build.
5. Detailed studies have been carried far enough so that we know fairly well its probable dimensions, costs, and products. There are enough gambles to make it exciting.

B. GLOBAL ENVIRONMENTAL MONITORING

The UN decision to convene a "World Conference on the Human Environment" in Stockholm in June 1972 has triggered vast new international environmental interest and awareness. Constructive preparations for Stockholm are going on in many organizations and in many countries. The UN challenge was to the governmental and non-governmental community alike, and both sectors are responding.

The questions involved in developing national policies to deal with international pollution-control are, however, highly complex—as are most things having to do with the environment. For some pollution problems, choices are difficult. DDT usage is a case in point. DDT appears in some countries to be the best hope for economically practicable control of malaria yet a ravaging scourge. In others it holds perhaps the only clearly realizable short-term prospect for increasing agricultural productivity in a country struggling for more food for starving millions. But the very properties of DDT that most commend its use, low cost and long-lasting toxicity, make it a more serious threat in the environment. It is easy to see why an outright earth-wide ban on DDT usage would seem unreasonable in many a developing country, however it might seem to us desirable

There are countless other examples. When pollution-free production of electric power, say, significantly adds to the cost of power production in a country where the cost is already beyond the means of most, concern with the environment seems a direct threat to widespread achievement of the benefits of development. Indeed, it is hard to see otherwise! It can only *be* otherwise, if the advanced nations of the world recognize the jump they have gotten at the cost of past pollution errors, and take responsible steps to aid the developing nations to achieve the costlier economic and industrial growth that will result from starting at the very beginning by protecting the environment.

The United States is now the world's leading polluter; we have visited our sins on our land and our earth for a long time. It will be costly enough for us to mend our own ways. It will be even more expensive for us to lead a world-wide movement to protect the environment, at the same time that we espouse bringing the fruits of development to all the world. But there is no other choice for us that I can see. An advanced nation will not get far in efforts to marshal world opinion behind environmental conservation, unless it can assure that industrial benefits will not be denied poorer nations by the higher costs occasioned by new international pollution standards. To a country struggling into the modern world, it may seem indeed an abstract worry that chronic eye and lung irritation, say, result from increased sulfur oxides released by the cheaper means of electric power generation that can bring higher standards of living faster.

Even in our own country, more depressed economic groups tend to be in tolerant of spending national resources on pollution control, when they have grave disaffections with their housing, jobs, schools, health care, or their compensation for work. Some even regard the environmental issue as a smoke screen for a do-nothing stand on their more urgent issues.

Our experience indicates that for all of their desperate desire for prompt access to the goods of advanced industrial societies, thoughtful

men in the developing nations are increasingly aware, as earlier arrivals on the industrial scene were not, of the magnitude of the long-term costs of doing things wrong. After all, it costs far less, in the long run, to avoid the problems of incorrect chemical industry plant-siting, for example, than to rebuild a city or an industry to get around earlier errors.

The human costs will, moreover, be far less if things are done right to start with, and direct experience with the error is averted. It is my hope that the developing nations will be eager, given credible opportunity, to benefit from our errors and to avoid them—particularly if we can share in the costs of their so doing, in an enlightened self-interest derived from our new-found knowledge that their natural environment is also, to greater or lesser degree, involved with ours.

In any event, it seems probable that the Stockholm conference may provide favorable circumstances for two important actions:

1. Adoption of a world declaration on protection of the environment proclaiming the rights of all men to enjoy healthful natural environment, and enjoining all nations to exercise responsibility in arresting degradation of the environment or wastage of its irreplaceable natural resources.
2. Development, at least in preliminary fashion, of program recommendations to establish a cooperative, worldwide environmental monitoring network for specified substances.

Already a number of national and international groups are involved in approaches to these matters.

I shall not seek to review the nature of all of the relevant groups or the character of the steps they are taking, but I will mention briefly a few of them:

1. The U.S. National Academy of Sciences, National Research Council, has created a Committee for International Environmental Programs, chaired by Thomas F. Malone, and staffed by Henry J. Kellerman.

This group provides reviews, in advisory capacity, of various national initiatives, governmental and non-governmental, and aids in the development of national viewpoints on environmental matters, including how best to structure an international monitoring network.

2. SCOPE. The International Council of Scientific Unions has for several years had a Special Committee on Problems of the Environment. This committee recently created a Monitoring Commission, which is in process of coordinating various national viewpoints about how to organize, within ICSU or the UN-related structures an appropriate environmental monitoring system, including such questions as what to measure, how to standardize measurements, where to locate monitoring stations, etc. The USA, USSR, and Sweden have been

particularly active in providing inputs to SCOPE's Monitoring Commission.

SCOPE has also proposed the creation of the ICE International Center for the Environment, for the purpose of establishing a global environmental monitoring program, an international research center on global environmental problems, and a central "intelligence service" or clearinghouse on environmental knowledge.

Other groups have proposed creation of an International Institute on Environmental Affairs (IIEA) as an independent non-governmental, nonprofit organization designed to stimulate decisions and actions in international environmental matters.

3. SCEP. A group called the Study of Critical Environmental Problems produced an excellent summer study with broad national participation, in 1970. The study, sponsored by the Massachusetts Institute of Technology, dealt in solid fashion with many aspects of environmental protection and reconstruction. The substantial report entitled: "Man's Impact on the Environment, Assessments and Recommendations" can be unreservedly commended. The SCEP group has also developed a series of recommendations for national and international monitoring. It identified specific pollutants for priority attention, including toxic heavy metals, ocean oil, heat dumping, and DDT-like substances. For each agent it discussed specific monitoring requirements.

4. The United Nations Association, about one year ago, created a special Environmental Group that has exchanged papers with a counterpart group in the USSR, and hopes to have joint meetings early in 1971 to see whether a joint position paper or perhaps an international paper can be generated as a non-governmental input to the Stockholm Conference.

Both groups discussed monitoring network proposals, and some discussion was given to establishment of world standards for effluents, and the matter of world enforcement, but the latter are only sketchily developed thus far.

5. The U.S. Government, of course, has named Christian Herter, Jr., Special Assistant to the Secretary of State, Mr. Rogers. This is, in part, a sign of the Administration's high priority of attention to preparations for Stockholm. Mr. Herter's office has no position, so far as I know, on global environmental monitoring.

6. Various other national and international groups have expressed interest and recommendations in environmental monitoring, ranging from the International Biological Program to the Smithsonian Institution research groups. The American Chemical Society, for example, has produced an excellent study entitled: "Cleaning Our Environment, The Chemical Basis for Action". It too is highly recommended.

In conclusion, it is clear that much talk about how to monitor is going on, but there is very little actual new research or development actively in progress to extend the various global environmental monitoring programs (CO_2, solar radiation, rainfall composition, ozone, atmospheric dust) that have been operated in somewhat uncoordinated fashion for many years.

One can expect that post-Stockholm concrete monitoring agreements will begin to take shape, and serious environmental monitoring will begin on easily comparable bases. However a great deal of solid research and development work is required yet. Moreover, stronger attention needs be given the frequently repeater expectations that indirect satellite monitoring offers substantial promise.

III. Other Environmental Science and Technology Issues of Special International Importance

It falls far beyond my skills or the time bounds of this panel session to conduct a full and comprehensive review of the problems engendered by man's intervention in the ecological balance of the biosphere. Instead, I shall select a few key problem areas, and even for these deal only briefly with a few salient issues. My discussion will be biased toward my own environmental interests and those of my fellow scientists in the National Center for Atmospheric Research, whose laboratory is the atmosphere itself.

A. MAN-MADE CLIMATIC CHANGES

Helmut Landsberg has recently published a superb review of man's effects on urban climates, and of his possible impacts on world climates. He points out that climate, the totality of weather conditions over a given area for an extended time, is variable. He then confronts the central evidences that man has already inadvertently altered local or global climates, or that he can in the future do so by design or inadvertence. I commend this highly readable article to you.

Landsberg takes a dim view of some of the exaggerated claims of climatic effects that have recently been debated in public media, often "with more zeal than insight." Among them he cites the seemingly groundless alarm over global oxygen deprivation.

He then proceeds with excellent objectivity to sort fact from fiction and substantive knowledge from speculation. Let me cite just a few of his statements.

A first fact is the well-established existence of substantial climatic fluctuations over a range of time scales and regions. Over the last 250 years, for example, in the regions bordering the North Atlantic, the late 18th Century was warm, the 19th Century was cool, and the first half of this Century was warm, with a marked downturn in the last

two decades. Average winter snowfall on Mt. Washington, New Hampshire, for the 15-year period centered at 1942 was over four feet less than that for the more recent, succeeding 15 years. But he warns against over interpretation of data like these as global or hemispheric indicators of climatic trends.

As Landsberg indicated, many factors appear to be involved in climatic change. Among these are tropical volcanic dust episodes like the great Krakatoa explosion of 1883 or the explosion at Mt. Agung (Bali) of 1963, each of which spewed vast clouds of light-absorbing dust and other materials into the stratosphere.

Anomalous ocean temperatures, and large-scale wind system trends appear also to be implicated in climatic trends, especially in shorter time scales. The severe eastern seaboard drought of the 1960's, for example, appears to have been accompanied, according to Jerome Namias by unusual ocean surface temperatures near the East Coast but even more importantly, by wind system changes far away in the North Pacific. These distant teleconnections, of course, are one of the reasons why the GARP program must be *global*.

In the face of these and the many other natural fluctuations that appear to affect climate, it is no small wonder that man's impact is hard to unravel and to know with certainty. But there are some suggestive signs of man's involvement. Landsberg gives details; I will only skim:

1. Carbon dioxide (CO_2)

CO_2 in the atmosphere, which is globally, is apparently increasing. Burning fossil fuels appears to be a major cause for the increase. Plants consume CO_2, but when they decay give it back. The oceans dissolve CO_2; but less so when they are warmer.

CO_2 is a governor of the retention of solar heat by the atmosphere (an effect often, and somewhat misleadingly called "the greenhouse effect"). It is thus possible that man is increasing the global CO_2, and that climatic effects result, and will be even greater as the CO_2 content accelerates upward. But without considerably more detailed research the matter must be considered speculative.

2. Dust

Volcanoes are probably the earth's largest sources of atmospheric dust. Deserts, like the Sahara, when whipped by high winds are also probably important. There is no question but that atmospheric dust alters the strength and character of the sunlight falling on earth, and so doing is likely to modify the climate. Man is, moreover, no inconsiderable dust maker. Not only by "breaking the plains" but also by industrial smoke, photochemical smog generation from cars, but by a host of other effects man and his cities are implicated.

In American cities, man's pollution has probably, over and beyond possible cloud effects, reduced the average incoming visible sunlight by about 15%; more in winter, less in summer. The reduction in ultraviolet light is substantially greater. When the city of London "cleaned up" by adopting smokeless fuels, winter sunshine increased 70% and horizontal visibility improved by an average factor of three.

3. Non-urban land use effects

Interestingly, Thomas Jefferson was the first of whom I am aware to suggest systematic climate monitoring surveys in newly broken farm lands to assess whether man's patterns of virgin land use would alter the nation's climate.

Soviet agriculturalists have long looked to forest strips and other means of microclimatic control to collect snow, ameliorate wind effects, and air farming. However, the effects of large irrigation projects on adjacent climatic regimes are still not established, save for the very boundaries of the lakes. Nowhere has changed land usage, to the best of my knowledge, been seriously proposed for large-scale climate effects. Speculations have been raised of the possible effect of changing the flow North Pacific to the Arctic Ocean by a joint American-Russian dam across the Bering Strait. What it would do, if anything, is still highly speculative. This kind of effect is a possible future topic for numerical simulation, as I described in the GARP section above. To try it would cost a few thousand dollars by simulation. In nature it would probably cost over $30 billion. This is why the suggestion cannot be considered serious.

4. Urban Effects

Every major city produces a "heat island." In addition to giving greater cloud-producing convection, this heat island can make urban temperatures as much as 10° F. warmer on calm, clear evenings.

It seems to rain more over most cities. Part of the effect is due to wind effects of the high buildings, which are somewhat equivalent to the well-known mountain effects. A 5% change in average winds at the top of the Eiffel Tower, as Paris has built up over the past 50 years, is, Landsberg suggests, one of the best direct evidences of this effect.

Another effect apparently evidencing man's impact is the fascinating evidence recently found for greater rainfall over cities from Monday through Friday, as compared to weekends.

Some evidence, still controversial, exists suggesting that man-made freezing nuclei and condensation nuclei released to the atmosphere alter snowfall and rainfall significantly. Automobile exhaust lead-sulfide, reacting in the atmosphere with industrial waste iodine or bromine, for example, appears to be a potent cloud seeding nucleus generator.

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Finally, so far as city-effects are concerned, the actual heat released to the atmosphere by heat-rejection by man is growing as power and population increase. Soviet scientists have expressed special interest in this.

Summer air conditioners and winter chimneys are major factors here. On a hot summer day a significant fraction of all of New York City's electric power is spent rejecting to the atmosphere the heat inside buildings. By the year 2000, Landsberg suggests, the heat rejection in "Bosnywash" (the Boston-New York-Washington megalopolis) may have an average winter heat-rejection rate equivalent to 50% of the total solar heating of the same region (a summer rate of 15%). Even the very human body metabolism heat loss from the 56 million people in the super-city will be a considerable contribution!

5. Supersonic jet effects

Much has been said about the possible climatic effects of SST deployment on a vast scale. Landsberg expresses the view that the small incremental amount of water vapor (one billionth part) added to the atmosphere would have no effect one could reasonably anticipate. He believes the environmental implications of sonic boom are far more important than any climatic implications. The SCEP summer study, mentioned in II B above, on the other hand, expressed concern over possible SST generated photochemical smog. For my own part I am inclined to agree about water vapor, but believe, with the SCEP report, that a careful technology assessment of the photochemical smog effect in the stratosphere is urgently merited.

I also believe additional attention should be given to nucleation effects of vapor trails left by sub-sonic jets in the far moister lower air where Boeing 747 and similar jumbo jets fly. I believe some large-scale climatic effects may result from increased cirrus cloudiness, though Landsberg doubts it. In any event, very little study has been made of effects. Here is an area where global monitoring might produce fruitful results.

B. WEATHER MODIFICATION RESEARCH

In several areas, deliberate efforts are being made to conduct research or operations to modify local weather. I shall largely skip over these because their international implications are not a major feature.

C. THE INTERNATIONAL BIOLOGICAL PROGRAM (IBP)

A highly valuable international program is in progress under the IBP. The The various biome studies, moreover, offer promise of value in assessments of ecological disturbance by man's future intrusions.

For some biomes we may have, as a result of the IBP work, ecological baseline measurements of considerable importance. (Literature is available.)

D. INTERNATIONAL HYDROLOGICAL DECADE

Another international environmental program with value to environment assessment and to climatic research. (Literature is available.)

E. BIOLOGY AND MEDICINE

To the best of my knowledge there is little or no internationally cooperative research in progress dealing with the crucially important subject of the medical and biological effects of exposure of humans and other species to various levels of environmental degradation. A number of innovative national programs with international aspects are, of course, in development. One of the most interesting, for example, is the Salk Institute's Council for Biology in Human Affairs, with international participation and programs under several commissions dealing with such topics as biology in international affairs (contraceptives, population dynamics, biological warfare, bridge-building to Communist China through biosciences, etc.).

F. INTERNATIONAL LAW

International environmental law is a field of probable great future importance. A number of studies are in progress, but I know of no internationally cooperative efforts in the field comparable, even, with the modest efforts in space law undertaken under auspices of the International Federation of Astronautics.

Weather modification law is, of course, a small but hot field in the U.S. today, and so is the growing domain of environmental law.

IV. COMMENTARY ON SPECIFIC ENVIRONMENTAL QUESTIONS OF INTEREST TO THE PANEL MEETING

A. INTERNATIONAL COOPERATION IN VARIOUS ENVIRONMENTAL SCIENCE DISCIPLINES

I have already commented on this matter above. To summarize in a few words, I visualize that the most active environmental cooperative thrusts will be in the Global Atmospheric Research Program, and in the realm of planning, organizing, and developing techniques for global environmental monitoring.

A major focus of interest now centers on the June 1972 Stockholm U.N. Conference on the Human Environment. A key issue at the Conference, and the pre-conference planning sessions is the real or imagined conflicts between environmental protection and the development

of the poorer countries. This may prove to be one of the most important world issues of the decade.

B. DEVELOPMENT OF NEW MECHANISMS FOR INTERNATIONAL ENVIRONMENTAL COOPERATION

1. Research program agreements

The time-honored pattern of voluntary agreements, variously formalized, is likely to continue as a major pattern for international environmental cooperation in the GARP program and other programs of the coming decade, in my view. The creation of the Joint Organizing Committee for GARP, approximately four years ago, was a rather innovative form of agreement, because it transferred all GARP-associated planning functions of the World Meteorological Organization, a U.N.-related governmental organization to a rather self-governing joint committee equally loosely controlled by a non-governmental entity, the International Council of Scientific Unions. The mechanism has worked well, with minimal bureaucratic red tape, and with the Joint Organizing Committee co-located with WMO, in Geneva.

Intergovernmental agreements are likely to remain the pattern for GARP, with minimal central bureau control. Much of the international work will be done by experts provided by various nations to the Joint Organizing Committee. The mechanism seems to function far more successfully than one might imagine, and the national experts "lent" to the international program appear to develop an excellent sense of identity and commitment to the integrity of the international program. It remains to be seen if this pattern will continue, but at the moment it looks propitious to me.

In respect to the Global Monitoring plans, there may be considerable push for more bureaucratic forms, with international secretariats, formal organizational identity, etc. In addition, there may be some jockeying for mission responsibility among the various relevant UN-related agencies with already large bureaucratic structures, like UNESCO, FAO, WHO, etc. Because of the diversity of disciplinary interests embraced by the problems of the environment, I would not be surprised to see nearly a dozen contenders for monitoring jobs. But such a form as that used for GARP might not work badly, with a small high-level policy and coordinating body giving overview.

2. An international science foundation?

The notion of an international science foundation pops up persistently in nearly all international policy discussions. Roger Revelle espoused it before the science policy hearings of this Committee last year. Considerable thrust appears to be developing for the concept here and abroad. The recommendation has much merit, to my view, in terms of environmental science. But Revelle's criteria should be observed:

small grants, developing countries, relevance to country need, opportunity for students, etc. It will not fill the need for major GARP-type research program support of international character. This must come largely, I believe, from governmental agreements, supplemented by smaller sums from international sources, such as the Development Fund administered by the World Meteorological Organization, or other international funds.

3. An international university?

Another appealing notion with growing numbers of adherents is that of a world university. If environmental science assumes the world importance that I predict for it, such a mechanism could play a highly useful role.

It might well have its origins entirely at the graduate research and educational level, and could be built around the notion at the outset, of a formally established coalition of developed-country University scholars (from different countries) in established universities or research centers (like the National Center for Atmospheric Research, the Sloan-Kettering Institute, the Brookhaven National Laboratory, or various university-related research centers) who would undertake association with small groups of scientists in each group associated with a university in a developing region, but governed by the World University. In each association, a tie to country need for the developing country would be important, and the effort should be made to do a single disciplinary group at each underdeveloped nation center, rather than to cover the whole range of science and technology at each place. Environmental science areas are, again, well adapted for such efforts.

4. Stimulation of multi-national corporate ventures in environmental science and technology

I am much impressed with the important technology transfer that sometimes materializes, under favorable circumstances, when multinational corporations take up the task of establishing industrial plants in developing regions.

In areas where the fruits of advanced technology are to be brought to a poor area, the sophistication to install environmentally-sound plants, the skill to integrate local managements, and to develop local markets in concord with local mores—these things seem often to be especially well done by multi-national corporations, and at small public cost.

I believe that attention might well be given to provision of special governmental incentives for the development of industry in environmentally optimal ways through multi-national corporate means. There is a discussion of this matter in slightly different context, in an excellent recent article by Caryl Haskins. (9) Perhaps Dr. Haskins could usefully be invited to develop specific comment on the adaptability of

this mechanism to the problem of having development *and* a clean environment.

5. Mechanisms for international environmental standard-setting and for control and enforcement

This is a knotty topic that I am not prepared to confront at this time. It seems to me that the first steps need be firmly started first, namely those of establishing agreed-upon parameters to monitor in the environment, and agreed-upon techniques for measurement. Control will probably be feasible only as a national matter, for some time yet, It seems unlikely that international pollution-monitoring or enforcement teams will yet be acceptable to most nations within their borders. However, it does seem likely that agreed standard levels for effluents will be achievable, with national control and enforcement kept within national authorities. In international territories it seems highly desirable to establish, as promptly as possible, the principle of international monitoring authorities. This should apply to oil on the oceans, organic wastes in international waters near river outlets, etc.

Space surveillance, if effective, may open new possibilities for pollution monitoring, under international auspices, over all countries.

C. THE LEGISLATIVE ROLE IN DEVELOPMENT OF INTERNATIONAL SCIENCE POLICY

In my view, the policy role of the public man is critical in international environmental science policy as in domestic—to echo McGeorge Bundy's last year's remarks.

1. Critical review and support of national programs

Programs like the GARP should be the subject of detailed and concrete Congressional review. I would like to see all elements of US participation in the GARP drawn together and made the subject of a full and coherent Congressional study and policy determination. As things are now done, parts of the GARP program are contributed by various institutions, governmental and otherwise, and it is very hard to give a unified picture of the real cost and the real merit of the program. To be sure, review is done in various interagency groups, and in all of the participating agencies, which identify and argue out their contributions in their own budget processes. But I feel that programs involving this measure of national prestige and world significance should be scrutinized at the legislative level in some sharper way.

I may be naive about this, but I would like to see a review of a major research program like GARP made in an appropriate single committee that has direct budgetary responsibility, rather than see a program like this pieced out from various contributions made by different agencies. It would help give an exciting program like GARP the public visibility that it merits. And it would give the Congress a sense of responsibility for the program that is now diffused amongst the various

groups. I am aware, of course, that the legislator has many demands on his time, so that very few programs could command this type of review and decision. But I think that it might be a good idea to try, in some selected environmental areas, and problably GARP is the only international example at hand now. The International Biological Program might have been a similar example a year or two earlier in its life. The IGY program, if my memory is right, did have such review and its own special budget.

2. Consideration of international environmental control standards and regulations

At the time when international environmental controls are established, by whatever form of agreement, the overall picture ought, I suspect, to be made the subject of an occasional in-depth panel review for the Congress. Perhaps the format of this panel meeting would be suitable, but with a much narrowed agenda.

Important policy matters to the national interest will be involved in environmental control issues, and the general public will have a very strong concern in the nation's position. So I believe it will be desirable to create some means of special communication between the Congress and the appropriate technical experts, well before we near the stage of freezing on international control standards.

D. RELATION OF ENVIRONMENTAL SCIENCE AND DEVELOPMENT TO DEFENSE AND ARMS LIMITATION

This subject has some highly interesting and important aspects. The use of herbicides and other plant regulating substances in warfare is but one facet. However, I have not addressed myself to these matters in the time available to me.

E. SPECIAL ENVIRONMENTAL CONCERNS IN THE DEVELOPING COUNTRIES

This subject has been covered in my comments above. To reiterate, the key issue here will be how to assure that environmental protection measures do not retard the advancement of the developing countries. The topic has far-reaching consequences, when we recognize that a change of life style is in prospect for all peoples of the earth if our resources are to be equitably distributed, and if every product manufactured is to be kept from producing environmental degradation of an unacceptable level or kind.

F. ENVIRONMENTAL SCIENCE POLICY AND THE SOLUTION OF SOCIAL PROBLEMS

1. Protection of public rights in environmental modification

New measures may be needed to encourage the protection of the interest of the public in subtle matters of environmental amenities or rights. Recent court actions, and the flurry of environmentally oriented

law groups, is a step in this direction. But there are many difficulties. The Trans-Alaska Pipeline and the nuclear underground blasting for gas development are good examples. So far, these issues are largely national, and so outside the domain of my paper. But their analogs are not far off on the international scene.

Where ocean fish, that make for one national group a livelihood, breed in another nation's territorial waters, and these waters are adversely modified, we have a small example of the kind of problem that may face us in larger scale.

In northern Europe there are now regions where severe air pollution is often present over a country, and has not originated in that country. International laws are going to be difficult to develop; there may be grave problems in protecting the individual. It may be hard to develop an international "breathers' lobby" like the one the Wall Street Journal recently identified in this country.

2. Priority setting for environment science and development

The priority setting problem is a tough one, for environmental sciences. How can one compare the importance of cleaning up Lake Erie with that of developing quieter, less-polluting public transportation in New York City? Science and technology, following the changing temper of our times, move increasingly toward domains that are relevant to social needs. But the choice between what social needs to serve is still the tough one.

Of one thing I am confident, in this field. I am convinced that we are better off with a diversity of sponsors establishing priorities, and making funding choices. But I also believe that each sponsor should not, by this token, do a less rigorous assessment of what he believes should merit top priority.

In all research programs, a reasonable long look is needed. Priorities should be tough, but large fluctuations in funding over a short time base are far harder on good science than hard times themselves.

V. The Peaceful Uses of Planet Earth

I would like to close my paper, already too long, with a brief comment which I have taken from a book I published last year.

Right after World War II many people believed that the fearful new weapon, the A-bomb, would force man to adopt a true one-world concept. The prospect of a world armed with nuclear weapons, poised to fall from the sky at split-second notice, anywhere over the globe, seemed just impossible as a way for rational men. The result was a wave of public interest in the notion of world government. Many forms of world government were proposed or discussed. Most involved a limited form of world government. Most accommodated cultural and political diversity.

Yet today, few seem optimistic about achieving a world control of warfare through a world government. The reality of intercontinental ballistic missiles, armed with multiple nuclear warheads, is with us. It is only a question of a very short time, as things now progress, till a half-dozen nations have the capability to arm themselves for overkill of their potential enemies.

The threat of war has not created a one-world world. We have not taken the steps that many anticipated would strengthen the United Nations into a limited world government capable of police action against military offenders. And the military budgets of the United States and the Soviet Union have grown, through mutual deterrence concepts, to huge dimensions that seemed unthinkable in the days just after World War II.

Perhaps, now, a new opportunity exists for realizing a world that is one. Perhaps the threat of inadvertent world environmental despoliation will prove a yet-stronger motivation for creation of limited world government. Many of the dynamic forces involved are different. For one thing, no one deliberately pollutes in order to harm his neighbor. He pollutes out of ignorance, or carelessness, or for short-term economic advantage. Moreover, many nations pollute, not just a few superpowers. And because of the fact that the air circulates over the whole earth, and does so rather quickly, pollution isn't the concern of just the contending super-powers. It falls on all.

By the next century, I believe, the conservation of the natural beauty and hygiene of the abode of man, our common planet, will provide a one-world motivation even stronger than the threat of war. If the mechanisms of world cooperation mature in order to bring about world environmental management, is it foolish to hope that they will be used, as well, to de-fuse the instrumentalities of war? I think not! I believe that it is a realistic prospect for man in the better world that Century 21 can bring.

INTERNATIONAL COOPERATION IN THE PHYSICAL SCIENCES

ABDUS SALAM [1]

Some 1,150 years ago, Mamoom-ur-Rashid, the fabled Caliph of Baghdad, wrote a letter to Leoi the Armenian, Emperor of Bysantium, which history has perserved:

> From the Commander of the Faithful to the Qaiser of Rum, Salutations. Know ye, that the Holy Prophet of Islam, blessed be his name, enjoined upon us, his followers, two sacred duties: acquisition of knowledge and its diffusion. Blessings of Allah be upon him, he instructed us: learning is the shared heritage of all mankind. He told us, it was the partaking of the fruit of the Tree of Knowledge which made sons of Adam differ from others of Allah's creations. In accordance with his injunctions, we affirm that ours, as the mightiest vice-regency of Allah on earth, owes more than any other, the obligation of creating new knowledge. We have thus ordained that an observatory be erected at Tadmor in the lands of Syria, to determine the inclination of the ecliptic, and to construct tables of planets' motions for computing the size of the earth. With your kingdom, since the Prophet's time, we have differed on religion and disputed on territory. But knowledge is our shared heritage So let those of your astronomers who will, come freely to Tadmor; they shall find the portals of welcome wide open. This site, a temple of learning, shall remain holy and inviolable. As an earnest of our knowledge-sharing we send you a new work in mathematical sciences, composed at our court, by that servant of Allah, Abu Abdalla Musa-al-Khwarizmi. This concerns a new path he has created in mathematics; he calls it Algebra.

History relates that Leon the Armenian duly received the first-ever test of the new science of algebra, he reciprocated by sending to Mamoon-ur-Rashid a collection of ancient Greek manuscripts. He could send no astronomers to Tadmor, because, in his dominions the state of mathematical knowledge did not go beyond the latest text on the use of the abacus.

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My intention in recounting this episode is not to contrast the state of physical sciences in Baghdad and Constantinople at that epoch of history. I wish more, to refer to the first part of Mamoon-ur-Rashid's letter, where he affirmed that, as ruler of the mightiest empire then on earth, he felt it was an obligation, laid on him by the Prophet's injunctions to encourage creation of new science, to invite for collaboration in this pursuit the most talented men he could find, and see that knowledge thus gathered was open to use for the benefit of all humanity.

I assume that the Committee's invitation to me to speak here springs from the same urge. The United States is today the greatest power on earth. Its national income is one-third of the world's total. It harbors within its confines some 40 percent of the world's scientific community. It is right and proper that the mantle of the Caliph Mamoon-ur-Rashid should fall upon it; both in respect of encouraging the creation of new knowledge and in taking a lead in making possible international collaboration in this pursuit.

My talk will have two themes: First, the need, in this age of rising cost of research in physical sciences, for international collaboration, this, to secure the optimal use of expensive tools of research—the best that humanity can provide—by the most talented people, irrespective of where these men come from.

And for this purpose I would like the creation soon of a World Science Policy Council for Basic Sciences.

Second, collaboration among all nations, designed to use existing science for the benefit of all humanity; and particularly developing humanity. This second part of my talk is broader than collaboration just in physical sciences, though I shall limit my remarks to this type of collaboration. This is to be fair to the physicists on whose part I talk.

I shall be particularly concerned with the as yet vaguely apprehended moral obligation of the international scientific community and its sponsors, toward building up of strong indigenous scientific communities in developing countries, so that these too can participate fully in the scientific and technological revolution of modern times. I shall be concerned particularly with the instrumentalities of collaboration, the world science policy for basic sciences I mentioned before; proposed international centers for research, both within and without the United Nations system; and finally the visionary ideal of one or more postgraduate science universities, internationally run.

To turn to my first theme, even though the whole history of science is a history of one civilization building on the science of another—Islamic science building on the Greek; European science on the Islamic—it remains a fact that, barring the rare instances like those at Tadmor which I recounted just now in the ninth century, direct international collaboration and participation in one and the same

scientific project is something of recent origin—possibly even a 20th-century phenomenon.

In physical sciences this has taken four forms:

1. The organization of global and international studies by the international scientific unions and the scientific agencies of the United Nations—I have in mind international programs like the International Geophysical Year.

2. Exchange agreements between academies, universities, and other institutions, international congresses, international summer and winter schools.

3. Organization of international laboratories and institutes in physical sciences like the laboratories of the European Nuclear Research Organization at Geneva or the nuclear laboratories at Dubna or, in a different category, the International Center for Theoretical Physics.

4. Finally there is the beginning of the collaboration in space research between NASA and European space research organizations.

I shall not take the panel's time in discussing in detail the working of the various instrumentalities of scientific collaboration like the International Center for Theoretical Physics at Trieste.

By and large, they have been most effective within the Western European or the U.S. context—less so when they have involved Eastern Europe or the developing countries. I shall, however, take time to be fair to my own community of physicists to speak of a need for serious—and early—consideration for collaboration in big science and costly science; a subject which did not, unfortunately, figure in the speech of the Secretary of State. As two examples of the fields I have in mind, I shall speak of research in thermonuclear physics and my own discipline—high energy physics.

Consider thermonuclear physics first. There is no question whatsoever that man's eventual energy source is going to be provided by the harnessing of the fusion reaction; by creating on earth the controlled conditions similar to those by which stars pour out their power. Our fossil fuel reserves are nonrenewable. If the whole world used them at the rate this country does, as the Secretary of State reminded us this morning, they would not last more than a few decades. And the present generation of nuclear fission reactors are dirty, degrading environmentally. Controlled fusion offers the final hope of clean, illimitable energy production, lasting so long as the deuterium in the oceans does.

All advanced nations are deeply interested. There is research work going on in the United States, U.S.S.R., Germany, United Kingdom, France. Most is unclassified; most is costly; each experimental device costs a hundred or more million dollars. With the Russian Tokamak, there is hope that commercial thermonuclear reactors may not take

longer than 20 years to build, if research efforts of the nations—the talents of their best men—were more consciously pooled.

This is an obvious case for international collaboration. This collaboration does exist—but only at the normal low-key scientific visits, and joint-seminar level. There is desire for more by those working in the field. But there are no modalities.

The only long-range collaboration that has taken place were the two workshops, one a year long, the other lasting four months. which my Center—the International Center for Theoretical Physics—was able to organize, and where 30 to 40 of the most talented United States, U.S.S.R., and West German theoretical plasma physicists met and worked together. At a panel meeting held last year at Trieste under the auspices of the International Atomic Energy Agency, the most enthusiastic plea for collaborative research came—not from the West—but from the Russian delegate—the renowned physicist Prof. Lev Arstimovitch. At least I felt there was need for greater reciprocation—from the Western side also.

Thermonuclear physics is a field where pure and applied physics join hands. Consider now the two frontier subjects of astrophysics and physics particles.

Since Mamoon-ur-Rashid's time—and indeed from times still earlier—the two frontiers of physical sciences man has been exploring are the large distance frontier, at present lying at quasar distances of the order of 10^{+14} cms., and the small distance frontier deep inside the hearts of nuclei, at 10^{-14} cms. When Mamoon-ur-Rashid spoke of man's chief glory being the tasting of the Tree of Knowledge, he was referring to exploration by his astronomers on one of these two frontiers.

Now as man has pushed his exploration further, the costs of doing this have proportionately increased. Radio and other telescopes to scan distant stars cost tens of millions of dollars; high energy accelerators needed to produce energetic particles probing deep inside nuclear matter cost hundreds of millions of dollars. The most powerful accelerator now being built—the 300 to 500 billion electron volt accelerator at Batavia in Illinois—will have cost some $300 million over 5 years of its construction with running costs of an experimental facility coming to around $60 million a year.

These are large sums of money to spend on what in the foreseeable future is no more than curiosity-oriented pure research, with at present the aim of glorifying of the human spirit. In GNP terms they compare with the sums the Middle Ages spent on their cathedrals erected to the glory of God. As one coming from the developing world, and one who speaks passionately on their behalf, I am frequently asked: Would I not like to see these sums diverted, for example, to their needs?

There is no doubt that I would like to see some hundreds of millions of dollars added to the 2 billions at present given in aid and loans to the developing world by this country. But I would most emphatically not like to see these sums made available from the pure science budgets. There are at present some $200 billion spent annually on humanity's behalf by those who run our affairs in an apocalyptic arms race—spent mainly to nullify the arms of one nation against another.

It is these budgets that I am hoping will one day be made available for world development, not the relatively meager pure science budgets which great countries like this can and must set aside—as Mamoon-ur-Rashid did to search for the great syntheses of truth, which pure science vouches and which technologically and otherwise determine the ultimate fabric of our human civilization.

In saying this I am being ungracious. The needs of the frontier disciplines in physical sciences have hitherto been generously provided for by the governments of the United States, U.S.S.R., and Western Europe, developing their national or group programs of research. Time, however, has come, on sheer cost, of size and scale, to consider well-knit international collaboration.

International collaboration in this field is not a new idea. It was first semiofficially proposed in 1960 by Mr. John McCone, then Chairman of AEC. This speech—10 years ago—led to a number of meetings under the sponsorship of the International Atomic Energy Agency with plans for a thousand billion electron volt international accelerator. Events have, however, overtaken those past initiatives with the emerging superconducting technology, the attainment of this energy is a definite possibility already at the U.S. national accelerator at Batavia. With this energy we shall explore the nuclear strata down to 10^{-15} cms.

But since the bedrock strata of particle physics are not likely to be reached—I am here using the language of our physicist colleagues in mainland China who have invented the terminology to describe this type of phenomena—We must contemplate 3,000 Bev or even 10,000 Bev accelerators. I spoke a short while back of great syntheses of fundamental sciences. The last great syntheses were the unification of space and time, and the explanation of structure of terrestrial and stellar matter in terms of quantum theory and the four fundamental forces of matter.

The next synthesis which I personally believe is coming—and in my personal view, already is on the horizon—is the syntheses of these four forces themselves—of gravity, nuclear physics and electromagnetism—the synthesis which Einstein dreamt of and failed to discover. In my view he failed for a simple reason. He did not live long enough to know that among the nuclear particles discovered by the Brook-

haven accelerator is a particle with identical quantum numbers as the graviton, the so-called F-meson. The nuclear force transmitted by F-mesons is identical with gravity, except that it is 10^{-44} times stronger.

Nuclear physics is strong gravity. Inside nuclear particles are regions of strongly curved space and time. Inside these particles, one may realize states of strong gravitational collapse—the black holes, so called, which Dr. Whipple and his colleagues may or may not find in the nuclei of galaxies. The microcosmos inside nucleons, in my view, mirrors exactly the macrocosmos of astronomy. To see if this synthesis exists we shall need higher energy accelerators.

It is at these energies that Mr. McCone's idea and his proposal for setting up of international accelerators needs reviving. An accelerator of energies of 5,000 Bev or higher—designed for the decade of the eighties—may cost several billion dollars to construct, spread over some 5 to 10 years. Its operating costs may be in the neighborhood of $200 million a year, roughly in the same order of magnitude as an international organization like WHO.

At costs like this, there is clearly need to share these between the West, the East, between United States, between Japan, and perhaps nationally between the developing countries. And to study the problem of such a collaboration there is need for the setting up of what I would call a world science policy council—a nongovernmental, small, but authoritative body of active scientists, covering eventually all pure science—but in the first instance, making recommendations in respect of worldwide collaboration in the high-cost frontier physical sciences I have mentioned.

Among its coordinating and recommendatory mandate could also be included another class of frontier experiments concerned, for example, with various types of cosmic radiations, including neutrinos and gravitation. The detection of such radiation is difficult. It requires construction of large equipment, with its location optimized at the scale of the whole earth—deep mines, high mountains, worldwide coincidence between widely separated detection devices.

The cost of these experiments is not high—a few million dollars at most—but it is the necessity to use almost the whole earth for detection which makes international collaboration such a prime essential for the success of these experiments.

There are some who feel dismay that science, pure science, should have become so costly, that its cost should transcend what one individual nation, even the wealthiest, could hope to afford. I am afraid I do not share this dismay. On the contrary, if one considers the future of humanity, confined on a small globe, interdependent as never before, living off a limited biosphere, which until recently we have exploited with no thought either for our neighbors or for our children, if we

consider that we also possess technological competence capable of providing plenty on a planetary scale if we were not hampered by the deadweight of our preatomic and preglobal traditions—if you remember all this, one should rejoice in costly science, rather than feel dismay.

Together with the problem of environment—which again very happily will admit of none but global solutions, big science and the need for collaboration in its pursuit are some of the best things that could have happened. The venues for collaboration which we shall develop, the modalities and experience we shall gain, shall hopefully be used elsewhere.

I hope very much that the setting up of a world science policy council is not long delayed. And this brings me to the second part of my talk, and that is the need to associate with any collaborative efforts the developing countries as full partners.

If past tradition is followed, whenever one speaks of scientific collaboration, one has had in mind the West, the East and possibly Japan. One has tended to leave out almost completely the third world—the two-thirds of the human family. Now, this is something, as you remarked, Mr. Chairman, which I have been fighting against in my humble way for the past 10 years, and with your permission, I would like to briefly speak to this.

I shall start my remarks with the subject I know best—theoretical physics. Theoretical physics is one of those advanced frontier disciplines where decisive breakthroughs have come in recent times not only from physicists of the West, East, and Japan, but also from those from some of the developing countries such as Brazil, India, Israel, Argentina, Lebanon, Pakistan, Korea, Turkey, and others.

Those who made these contributions received their training and, by and large, did their best work in the developed countries. This is, of course, not something peculiar to theoretical physics; almost every scientific discipline has in the recent past received front-ranking contributions from men from developing countries.

What distinguishes theoretical physics, however, is that in addition to these first rankers, there are in a fair number of developing countries sizable, potentially self-sustaining communities of theoretical physicists who could set up viable centers of research in their own countries if they had the right conditions for flourishing.

One of the major reasons why such communities do not flourish is related to the unfortunate fact that geographically most developing countries are far from those which are scientifically active. The result is that these communities remain isolated. The greatest enemy of creativity for those living in developing countries is their isolation. If active physicists working in the developing countries could come frequently to advanced centers, not to stay permanently, but to renew

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their contacts and go back refreshed after a period of concentrated research, this would solve perhaps the most frustrating problem hampering the growth of indigenous scientific communities in these countries.

My thesis is that the provision of such contacts is the responsibility of the world's scientific community and its sponsors. It was to provide such contacts in an organized manner that we conceived the idea of setting up an international center for theoretical physics under United Nations auspices.

The idea of setting up such a center was in some degree sparked off by Mr. McCone's speech, which I mentioned earlier. Reflecting on his speech, one felt that it may be some time before collaborative international experimental accelerator laboratories could be set up. However, it might be possible to start with something more modest, for example, a truly international center for theoretical physics.

In September of 1960, I had the privilege of attending the General Conference of the International Atomic Energy Agency as the delegate from my country. With the cosponsorship of the Governments of Afghanistan, the Federal Republic of Germany, Iran, Japan, the Philippines, Portugal, Thailand, and Turkey, we introduced a resolution on behalf of the Pakistan Government suggesting the setting up of a theoretical physics center under the auspices of the IAEA. In consonance with the standard United Nations practice, the resolution started with a preamble which stressed the unique virtues of theoretical physics for peace, prosperity, and health of mankind.

It took us 4 years of very intense effort to get the Board of Governors of IAEA to set up such a center for a trial period of 4 years. This finished in 1968. The Center's life has been extended for a further 6 years, with UNESCO joining hands as full partner with the IAEA in financing and running the Center from 1970.

The Center receives as visitors, leading physicists—in principle from 100, but in practice from around 60 nations—40 of them developing These men come to work on their own research problems in the milieu which they themselves create. In this, the Center does not differ from any other research institute in theoretical physics. It does differ, however, in two important respects from normal institutes.

First, being a United Nations institute, its operation is fully supported—and this is an important point to remember—fully supported and sponsored by Eastern Europe and it is not affected by the day-to-day political temperatures of the world. In fact, we have special federation agreements which really function with institutes throughout Eastern Europe. The Center is perhaps the only research institute where, as I mentioned before, even in a sensitive subject like plasma physics, we can bring together teams of leading people from the East and the West to meet for a year or more without any worries about the

political sensitivities which normal collaboration of this type usually entails.

Second, the Center is distinguished by the easy formality with which it arranges long-term contacts of physicists from developing countries. To break the barrier of their isolation, the Center has pioneered what it calls an associateship scheme. I shall describe it, for I believe it possesses wider applications outside the Trieste Center.

An associate, in our terminology, is a physicist working in a developing country who is simultaneously a member of the Center's staff. Once elected, he can come to the Center every year for a period ranging from 6 to 12 weeks, with no formalities except a letter announcing his arrival. With a generous grant from the Ford Foundation, the Center pays the associate's fare and his expenses at Trieste. We have of the order of 65 associates at the present time. If I could have money for 200, this would cover any theoretical physicist of any standing who should be associated with the Center in any respect.

The associateship lasts for 3 to 5 years and is renewable. The intention is to try to cover all top active men in the developing world and to give them this financially guaranteed possibility of remaining in touch with other leading men in their subjects. The crucial feature of the scheme is the stability of the 3 to 5 years which it provides for a leading man in a developing university to plan his work and career. The one important prerequisite for remaining an associate is that 9 months of the year must be spent in the developing country itself.

There is nothing special about theoretical physics so far as schemes like associateships are concerned. There is no reason why other institutes in other sciences may not start similar schemes to ours to end the problem of scientific isolation. Every university in the Western world could do this—five to 10 associates in any subject—ending once and for all the problem of isolation of scientists from developing countries—which in my view is the major reason for brain drain.

The United Nations Advisory Committee, at a session in New York 2 years ago, took the initiative to convene a meeting of the U.S. university presidents and heads of U.S. private foundations to set up, in conjunction with the U.S. National Academy of Sciences and the Canadian Research Council, a scheme catering in this manner for between 200 and 300 scientists in all subjects.

I believe the Canadian Research Council has already started its scheme of associateships or dual appointments of those leading men who have in the past received their research training in Canada. The U.S. action, however, was hampered by there being no funds of the order of a million dollars or so at the disposal either of the National Science Foundation or the National Academy of Sciences to finance such an anti-brain-drain device.

I am frequently asked, I am speaking here of pure science—I have been speaking in particular of theoretical solid-state, high-energy, plasma and nuclear physics—these are the Rolls Royces of the sciences—the question is often raised: Are not these pure sciences, and the urge to create them luxuries so far as developing countries are concerned?

My answer is "No," and I would like to go into this for a few minutes.

The shape of the human community, our modern mode and organization of life, is so much a product of modern science and technology that science, and particularly technology, is the concern of every one of us—whether he comes from a developed or developing country. Take my country, Pakistan, as an example.

Pakistan has few natural resources. On present level of prospecting it possesses no metals, no minerals, very little oil. Our major resources are three: (1) natural gas; (2) rich alluvial soil, provided it can be irrigated in West Pakistan and protected from floods in East; (3) abundant man-power, provided it is skilled. These skills include agricultural, technological, commercial and managerial skills needing high literacy, in sciences, in Engineering in Mathematics. Highly skilled man-power is needed for (a) protecting the country's territorial integrity; (b) for ensuring agricultural plenty; (c) for all manufacturing—in fact for ensuring any sort of honourable existence for Pakistan in the modern technologically competitive world.

The abundance of agricultural riches which our soil's bounty provides us in West Pakistan owes its origin to the superb irrigation system—the largest in the world, irrigating some 23 million acres—which the British left us as a legacy of 19th century dam-building technology. Our burgeoning population is a direct result of the antibiotic revolution—the antibiotic technology of the postwar period; our 3-year-old self-sufficiency in food is the result of the green revolution—inaugurated by Dr. Borlaug and his colleagues—and fertilizer technology.

Finally, for solving the most pressing problem of Pakistan—that is, for limiting our population—we are waiting for developments in steroid technology—specifically for developments which will bring down the cost of contraceptive pills from $1 a month to 5 cents a month and then it will be possible to distribute them so they are effective in bringing about a real population control which everybody talks about, but doesn't put his finger on the real problem, which is simply the cost of contraceptive technology. Being so specific, it is completely out of reach of the mass of people in Pakistan.

In the last analysis, technology, ultramodern technology, science, latest science, affects the humblest worker in Pakistan as much as a citizen of this great country. The time lag in the making of a relevant

technical advance and its application is a matter not even of months, but of years.

Now this would seem to argue against my thesis—that developing countries need more science—that by suitable mechanisms their first-rate scientists—and these exist—should be made equal partners in international scientific collaborations. On the basis of what I said about technology, one could well take the attitude that all that our countries need is imported technology—the know-how itself and the training in how to utilize it.

Unfortunately things are not that simple. The truth is that no amount of imported technology without very strong indigenous backing—as for example was assiduously cultivated by the Japanese society—will lead to a long-term solution of the problem of under development. The developing world has been importing black box technology for at least half a century. If anything, this has magnified their underdevelopment.

Technology, in order to get assimilated into the fabric of a society and in order to become part of its tradition, needs a strong local base of scientific knowledge and skills. Before a true technological revolution can come about, this base will have to be created. And in another room, perhaps, I believe Prof. Odhiambo will be pleading for the same thing.

And for the creation of this base, for the creation of the instinct of what is credible and what is not, what is scientifically and technologically genuine and what is deceptive, there is nothing as potent as the direct experience of living science—living in one's own conditions and environment, and flourishing within one's own cultural tradition.

We do need within our countries men who have the profoundest scientific, profoundest technological comprehension of what we technologically import. And just to support this class of man, a country like Pakistan needs some first-rate basic science—and men who create it—in its universities.

To get the scale right, there are some 2,000 creative mathematicians in the United States; Pakistan's population is half the U.S. size; its income is one-hundredth of the United States. On the first basis we should target at a thousand research mathematicians; on the second criterion our target should be 20.

Pakistan's present score is two, and this for a total population of some 120 million. These are the men who between them range over for us the entire gamut of mathematical knowledge—computing, advanced mathematics, advanced hydrodynamics needed for all engineering, all science, all oceanographic studies, all cyclone warnings, and the like.

How to produce men of the type I have described in Pakistan—and elsewhere in developing countries—how to keep them there and how

to keep them creative, even though they form very small groups? This is the problem whose solution I would like to see the international scientific community and its sponsors take upon themselves as their charge. I believe it can be solved by international scientific collaboration perhaps in the manner in which we have tried to solve it in Trieste.

To my mind, there is no escape from the founding of one or more international universities for coping with this and like problems in the manner of the Trieste Centre. That 25 years after the founding of the United Nations we are still nowhere near foundation of a United Nations International University is no tribute to the world's academic community.

I am thinking particularly of a postgraduate scientific university. Fortunately a design for a gradually evolving university could be imagined, with which one could link the associateship idea I described earlier.

One may envisage the creation of a world federation of existing international institutes for advanced study which may constitute a first step towards achievement in the future of the bigger ideal of a full fledged world university. The essential element of the plan would be to identify presently existing research institutions in the United States, in Western Europe, in Eastern Europe and even in the developing world which are already international in character and to strengthen the international aspects of their program. All such institutions may pledge themselves to make their faculties as international as possible, within their charters, reserving perhaps 20–25 percent of their resources and facilities toward supporting the work of high-grade scholars from developing countries and from countries of other political complexions, through dual appointments or other devices.

As I said earlier, in time this Federation of Centres of Excellence would become the nucleus of the World University I have been speaking about. I know the Soviet Academy of Sciences is in favor of the federation idea, as indeed are a number of first-rate institutes around the world. What is needed also is an indication from the side of the West.

Finally I shall mention just one more initiative which could have enormous potential in raising the level of scientific research in developing countries. In this country, a large proportion of postgraduate research in the universities is supported by grants from the National Science Foundation, Atomic Energy Commission and similar bodies. I do not know the precise figure for such grants here, but I do know that in the United Kingdom some $100 million are thus made available from public funds for university research in the form of grants to individual scientists.

Unhappily, in most developing countries this type of support is unknown, mostly through lack of resources. In Pakistan, for example, I know that the total funds available for scientific research in all the 12 universities amounts to no more than three-quarters of a million dollars. There are no international agencies at present to cater to such needs. The aid organization of the United States has never had it in its mandate to help pure science or even technologically oriented science in the countries it operates in.

The UNDP has, since last year, set aside $3 million annually for such assistance—some 1 percent of its resources, still not a vast sum. UNESCO and IAEA, as Professor Buzzati-Traverso will tell you, are impoverished organizations, they are small, ill supported by the big powers.

This leaves the great U.S. private foundations, Ford and Rockefeller, who do a superb job, in the context however of help to larger projects. There is no one looking after the needs of the small investigator.

Together with Professors Marshak, Auger, and Roger Revelle, we proposed at the United Nations Advisory Committee meeting of last year that an International Science Foundation should be set up with the same tasks as, for example, the National Science Foundation in this country. Assuming some $5 million were available annually, the International Foundation may function through a series of national subcommittees making individual grants, like the NSF.

We have envisaged the International Science Foundation as a small grant giving body, directing its activities to developing countries only. It will be a nongovernmental organization; on its board there would be representation of the world academies.

Could this Foundation take over other roles, like, for example, chartering world science policy in respect of international collaboration in its general aspects? There is need for such an organization, as I suggested in the first part of my talk.

To summarize what I have said:

1. There is need for greater funds to be made available for collaboration in physical sciences—other speakers will doubtlessly speak of of the need in their own sciences; I have pleaded for my own subject—high-energy physics. There is need for advance thinking for the decade of the eighties. For this we need group planning, and one hopes this great country in that respect will take a lead.

2. There is need of recognition that in all collaboration in science those from developing countries need to be treated as equal partners.

3. There is urgent need for setting aside for direct international cooperation funds to cover the U.S. contributions to the setting up of international centers, the World University, the International Science Foundation and other such initiatives.

4. There is need to strengthen the U.N. Agencies dealing with science; the United Nations, that much maligned organization, is the one place where the entire family of Man meets to discuss and plan humanity's future on the basis of equality, and this is something from the developing countries and from the East which is prized very, very highly.

I started my talk by recounting one international endeavor in pure science. I shall end by recounting one more. This relates to the conditions of developing countries.

Seven hundred and fifty years ago, an impoverished Scotsman left his native glens to travel south to Toledo in Spain. His name was Michael, his goal to live and work at the Arab universities of Toledo and Cordova, where the greatest of Jewish scholars, Moses Ben Maimoun, had taught a generation before.

Michael reached Toledo in 1217. His interests lay in the sciences of astrology and alchemy, then fashionable in Scotland. But once in Toledo, Michael formed the ambitious project of introducing Aristotle to Latin Europe, translating not from the original Greek, which he did not know, but from the Arabic translation taught in Spain.

Toledo's school, representing as it did the finest synthesis of Arabic, Greek, Latin and Hebrew scholarship, was one of the most memorable of international essays into scientific collaboration. To Toledo and Cordova came scholars, not only from the rich countries of the East like Syria and Egypt, but also from developing lands of the West like Scotland.

Then as now, there were obstacles to this international scientific concourse. First, there was the political division of the world. In 1217 the wounds of the Third Crusade, fought barely 30 years before, were still not healed. And then there was the economic and intellectual disparity between different parts of the world.

Men like Michael the Scot and his contemporary in Toledo, Alfred the Englishman, were singularities; they did not represent any flourishing schools of research in their own countries. With all the best will in the world, their teachers at Toledo doubted the wisdom and value of training them for advanced scientific research. At least one of his masters counseled young Michael to go back to the clipping of sheep and weaving of woolen cloth. Fortunately for the future of Western humanity, I am very glad Michael did not.

There are some practical problems in trying to start such a thing as an International Science Foundation, not from the standpoint of rich countries, but from the standpoint of the poor ones.

My own feeling is that we could probably get over these difficulties. First of all there is the problem of getting the Pakistan government to look after its own scientists. The 12 universities of Pakistan are

at the moment budgeted at three quarters of a million dollars for the entire scientific research program.

I have no doubt whatsoever this won't be changed in the near foreseeable future. People are not convinced.

Persuading the Pakistan government to increase the three-quarters of a million dollars to $3 million is not going to happen. It is an unfortunate fact. Scientific research is considered peripheral.

One big problem is red tape. At The Centre in Trieste, which is a U.N. operation, we have this problem. Those of you who are familiar with the U.N. operations know very well the routing that the U.N. documents have to have.

For example, for my fellowships, the procedure is the two director generals, one of IAEA, and one of UNESCO. They are jealous. I can't sign the document which goes out announcing these fellowships. They must sign it. It takes about a month to get one of them to sign. Anyhow, it is signed.

Then it is sent to the Government. It resides in the Atomic Energy Commission in one case and sometimes never gets to the university. If somebody gets one of these fellowships, he is supposed to get these commissions to agree to send the application to us and then we have to go to Vienna or Paris to make a selection. A large panel sits for the award of ten fellowships. Twenty people are involved, and then it goes back through the same routes to the government and so on. We follow that rigorously. United Nations procedures, incidentally, are decreed by the foreign offices of the countries concerned. It is not the U.N. that is responsible, it is the foreign office. This is followed.

But we have found ways of cutting it completely. We bring in 800 copies of the announcement before it is signed. We send it on our own initiative to all the unviersities concerned. We ask all the applicants to send in two copies, one straight to Trieste, and one to Vienna. From that long list we make the selection and inform all the people half a month after we have received the application.

We devised the following formula, the government is informed that the international center chooses to confer the privilege and honor on such and such a national of their country; if within 10 days we do not hear a "No" from the government, the thing will be awarded. Now this can be done.

We are doing it. Only one government objected. We took no notice. No heavens have fallen. This includes some of the Eastern Europeans, some of the tough countries. I have no doubt that between us we shall find some mechanism even to deal with Russia; if we find the country tough, I have no fear.

NEW MECHANISMS FOR SCIENTIFIC COOPERATION IN THE FUTURE

VIKTOR A. AMBARTSUMIAN [1]

I appreciate very much the opportunity to speak before you on the
problems of international scientific cooperation.
This is, of course, very important, and at the same time a very,
very complicated problem. I expect only to contribute a little. The
presence of my friends, who work very much with me on these prob-
lems of international scientific cooperation, gives me hope that we
shall gradually progress in the solution of the situation before us.
I represent the International Council of Scientific Unions. You
understand that the International Council of Scientific Unions, or
simply ICSU, considers the problem of better international scientific
cooperation as its most important task and as the major problem for
which it is called into existence.
You know that ICSU is the biggest non-governmental organiza-
tion of scientists. It is a real union of special scientific unions, such as
the Astronomical Union, or the Mathematical Union, or the Inter-
national Union of Pure and Applied Chemistry, and others.
We have 16 such member unions in our council, and at the same
time, among its members, ICSU counts many countries. The member
countries are represented by the corresponding academies of sciences,
or the research councils. And of course, all the problems of the inter-
national scientific cooperations are very important for us, ICSU.
You and everybody understand that international scientific co-
operation has great significance for the development of the science as
a whole. Of course, science as a whole profits very much from such
cooperation, but everybody agrees that it also brings profits for the
development of science for any given country, both for developed
countries and the countries which are now developing.
It is equally profitable for the countries with high scientific poten-
tial and for countries which, at the moment, have low scientific poten-
tials. Therefore, we must agree that international scientific coopera-
tion is a progressive factor in the life of humanity. And everything
which is against it, every factor which can destroy such cooperation,
is of course to be evaluated as a negative factor, for example, political
tensions, [illegible] ational discriminations.

[1] Presider[illegible] [illegible]onal Council of S[illegible]ntific Unions, Rome, Italy, and president, Academy of [illegible] the Armenian Soviet Socialist Republic, Yerevan, U.S.S.R.

n the other side, we know that the cooperation in science brings, result, good relations between scientists, and in this way it helps ring understanding among the nations, among the peoples of the d.

old times, let us take the 18th century, there were no difficulties nternational scientific cooperation. It really existed, when the nce was concentrated in several, mostly European, academies. It sufficient to write scientific letters to each other, and exchange the lications; this was sufficient for the science of that epoch.

ut now, the problem is much more complicated, owing first to the that science has spread over many, many countries. There are ny, many countries whose scientific potential is very large. At same time, science itself has diversified; it has many new branches, really, at any given moment, we can say that new branches of nce are coming into existence. Therefore, the problem is very comated, since almost in every case, whenever you are making any step ard international scientific cooperation, you meet multidisciplinary blems.

At the present state of this cooperation, we are implementing interional cooperation, by means of the so-called international scientific ons, which, of course, are very effective. Some of them are espelly effective—particularly in the fundamental sciences. I speak ay mostly about the fundamental sciences.

f we take the fundamental sciences, I can say that the existing ions do magnificent work. There are some examples of such unions. the union to which I personally belong the Astronomical Union, ey work very well, implementing the ideal of scientific cooperation tween the countries, between the astronomers of the world.

However, as I have said, we have the problems of cooperation in the erdisciplinary field. These problems are of special interest for the SU, since ICSU is the union of unions. It is responsible for the ganization of international activity in such interdisciplinary fields.

It is a very happy situation that the Academies of Sciences of differt countries are members of the International Council of Scientific nions This helps in organizing the work around some interdiscipliry programs.

ICSU has initiated a large number of programs which are well own. For example, we had a successful program for the Internaonal Geophysical Year. We had a program of the International Year the Quiet Sun; we have now—it is in final stage—the International iological Program, the first major program in biology. And now, we ave entered into the Global Atmospheric Research Program that, gether with the World Meteorological Organization, will embark n studies of the world environment.

Of course, the problems of environment are very important practically, but we take the responsibility only for the scientific side, trying to solve the scientific problems of monitoring and methods of work.

Now a special committee on the environment is organized, and in many countries, we have already national committees, who help with this international effort. We can hope that in this program, we will be as effective and efficient as we were in other programs.

I shall mention here also a very important program about the possibility of a world system of scientific information. The study of the possibility of such undertaking was made by ICSU, together with UNESCO, and this whole study was headed by Professor Harrison Brown of Cal-Tech, who is present here. This study has brought very substantial results. And we think that now we can hope that such a program, such an undertaking, will be implemented, and that we shall have cooperation in building a world system of information.

I mentioned this study of the possibility or the feasibility of the world information system, not only as an example of a piece o rk which is done by ICSU, but also to tell you that the study has been planned and that the people who worked on it are trying to find the optimal soultion of this problem.

In the case of the general problem of international scientific cooperation, of course, no such study, before the beginning of the cooperation, has been made. We scientists are pragmatic. As I have told you, our predecessors started with international cooperation in science, in very old times, and while we are gradually changing the forms, mostly we are going in a very pragmatic way. But perhaps this way, this method of work, which gives many possibilities, also has many deficiencies.

As I have told you, we have, and are beginning, from time to time, new and international undertakings, like geophysical years, or the environment programs. We begin one program after another. This is happening, according to suggestions of some scientists, of some national committees for ICSU, or of some union, but we are not proceeding in a very scientific way.

Therefore, one of the major problems now is the following: The whole field of science is before us. We must study the situation, in science, each year, with the aim to find the points where the international cooperation, multilateral cooperation, is important, where it can bring good results. Then we must proceed to work around those points, in the corresponding fields.

Now it is necessary for ICSU, and not only for ICSU, I think, but also for other unions, to study the situation in every science and the situation in the whole science, and interdisciplinary fields. We must find the hot spots, where large scale international cooperation is necessary, and organize such international cooperation as is required.

This idea was the result of discussion we had during the last meeting of ICSU officers, and we are going to try to change our work according to this idea. Perhaps this will bring more optimality to the organization of international scientific cooperation.

Perhaps it is correct to say that until recently, when we were meeting in ICSU we were spending all our time on problems of the organization of ICSU itself, how to organize the work of the meetings, or of the executive committee itself, et cetera. But now we have recognized that the most important thing for the ICSU, for its Executive Committee and for its leadership, is to study the situation in all of science, and then to find the problems which urgently require international scientific cooperation.

As I have said, the implementation of scientific, multilateral, and international cooperation is the aim of the unions. We have already many good unions. However, when speaking about international cooperation, we should not forget the importance of bilateral, or trilateral cooperation. Bilateral cooperation between countries which have well developed science brings good results, since we know that usually countries are progressing in science in different ways. Therefore, they can very well complement each other in the development of science. I shall bring in some examples a little later.

You will excuse me that I will bring the examples from my field. It is quite natural, since everybody can speak only on the problems which he understands.

There are cases where the different countries can complement each other in such a degree that cooperation between them can bring very great advantages. It is necessary to have some mechanism which can endorse, help, or initiate organization of such cooperation between two countries, or three countries, I was impressed by today's report by Doctor Low about the possibilities of cooperative work in space between Soviet and American scientists.

It is very well, when scientists meet, but of course there are many needs and many other possibilities of concrete common scientific work. I think that the international unions, and the ICSU itself, must not forget that many things are possible to do. Many aims are possible to achieve, when such bilateral cooperation starts, and therefore, the unions, at least in some cases, can stimulate such cooperation between the scientists of two or three countries.

This is a field which is mostly forgotten in the activity of our unions. Despite the title announced in our agenda, I can't find or invent completely new methods of scientific cooperation but I think that it is time to put emphasis on some methods of cooperation which are neglected, or not sufficiently exploited.

There are two reasons for having bilateral cooperation between countries. The first is that sometimes the scientific equipment of one

country complements the scientific equipment of another country. As a good example. I can cite here the case where the French scientists are preparing a very large bubble chamber for the experiments with the Serpuklov accelerator.

I think both the accelerator itself and the bubble chamber of such large size are so costly, are so difficult to build, that cooperation in such cases is quite natural, between any two countries which can in this way complement each other. Of course, the equipment is very important for science, but I think the scientific people, are more important, here I would like to express the following idea: When we consider two such countries as the United States and the Soviet Union, owing to many facts, mostly for political reasons, the scientific exchange which exists is not so great as we desire—as I desire, and I am sure as is desired by many Soviet and American scientists.

Of course, we have some cooperation, since we are sending each other the reprints and books. We meet in conferences, and even are doing some work together; not sufficient, but we are doing it. However, it is desirable to do more, because there is some degree of isolation between the scientists of the two countries.

Exchanges are not so frequent as we desire—especially common work on definite problems and common investigations. Therefore, the very mood and way of scientific thinking, in such situations, is in some degree different in the Soviet Union, in the same field. Thus, in the same field, you meet the people who are differently minded. They have different ways of approaching the problems, and in such cases, of course, the meeting of these people, when they work on a common problem, is very important.

There is a Russian proverb that there is no evil without good. That is, you never get anything good without getting something bad.

We are a little dissociated, though not completely. We don't have, simply, homogeneity of thinking, there and here. This shows that we can complement each other. This is my point.

When I speak about the different cases, of multilateral and bilateral cooperation, you must take into account that those problems which are mostly connected with the earth, its atmosphere, and the oceans require multilateral cooperation—really international cooperation in its broadest sense.

But if we take other programs where the ways of solution are not so clear, there the true bilateral cooperation often is better, and can better serve our purpose.

Can I bring you one example of the cooperation in astronomy? You know that here in the United States you were very successful. You are proud of the fact that you have built successfully a number of the largest telescopes in the world. You already had the 40-inch telescope

in existence at the end of the last century. Then you built the 60-inch at Mount Wilson, then a 100-inch, and then a 200-inch in Palomar. Increasingly these made possible the observation of the very faint stars, and the very faint and distant galaxies.

All these very large telescopes have the ability to concentrate on the investigation of small star fields and of the spectrum of a single star only. At the same time, the number of stars which are now in our reach, with these telescopes, is measured by billions, and millions of galaxies. We can study them by means of these large telescopes.

You here in America have tried to meet the difficulty of choosing between the billions of stars, or millions of galaxies, and the stars of the galaxies which are especially interesting. You have undertaken the program of photographing the whole sky by means of a Schmidt telescope, which has a broad field.

The Schmidt telescope of Palomar is very successful. You have obtained, by means of these maps of the sky, information about the positions of millions of stars, and some possibilities to have preliminary information about the physics of stars.

But now the requirements are more strict, and therefore we have at our observatory in Bjurakan, in Armenia in the Soviet Union, the means to work in this field. We have built an instrument of the Schmidt type however with an objective prism, which gives us the possibility to have at once, photographs of thousands of spectra of stars, with very small dispersion. From them we have a very small amount of spectral information, but this information is much higher than what we can obtain from the images, the direct images of stars, without spectra. Then I can say, "Oh, this star has an interesting spectrum," and conclude about the necessity of studying it with a larger instrument, with greater dispersion, and to follow it.

In this way, we now have the possibility of choosing among the stars, or among the galaxies, the most interesting objects. Part of this program is already being implemented, and the new types of galaxies we have discovered at our observatory in Bjurakan, now are under study; the lists are available; we have made them available to your observatories and it has been a great success. Many of them have been investigated by your astronomers with greater telescopes, via an exchange program, coming from our country to your country, and this showed very interesting results. We have found many galaxies of the intermediate type between the usual galaxies and quasars.

This is one of the interesting examples. After some time, it is possible that our roles will change. We are building a larger telescope, a 6-meter telescope, but our Schmidt telescope will be insufficient to obtain initial data about the fainter stars.

Now if you in the United States will build, as Dr. Zwicky has proposed, an objective grating for the 48 inch, you will have, then, an

instrument which can go deeper, in exploration of fainter stars. Then you can provide the initial material, to find where are the interesting objects, among the still fainter stars, and we can, in the future, observe them when we shall have our 6-meter telescope. Thus it may be that we shall change our roles. But, in any case, this example shows how easily we can complement each other in some fields.

This is an example from astronomy, but I think that we can have the same situation in many other fields, especially in the branches of science where you require large numbers of very expensive instruments; astronomy is such a science. We require a large number of very large telescopes in order to study all these very interesting and very faint objects.

Thus it is possible to have cooperation, especially in order to complement each other. For example, I think that this remark is true not only for ground-based astronomy, but for space astronomy also.

I have taken much time, but may I tell you something about science in the Soviet Union, especially in the field in which I have some experience.

I think it is interesting especially for the Americans. You know that we have problems in the coordination of scientific work, not only international, but also within countries. It happens very often that in two universities of the same country, or in two scientific institutions, people are doing parallel work, not always having the necessary exchange. It happens also in our country.

In the Soviet Union the investigations in the fundamental sciences are concentrated in the Academy of Sciences itself. The institutions which are making the most important scientific work in the fundamental sciences belong to the Academy of Sciences of U.S.S.R. or to the academies of the Soviet Republics, of member Republics of the Soviet Union.

Since the time all these academies of the member republics have been established, we have seen the problem of how to coordinate their work. However, it was not very difficult, since all the problems of finance, the distribution of funds between the different disciplines, are in the hands of scientists, more precisely, in the hands of the academies of the republics. Therefore, the problem was only to coordinate the efforts of the academies of different republics.

This is in some degree similar to the problems of international scientific cooperation—with one difference—here we have no political difficulties. At the beginning, when all these new academies of sciences began to work there were some difficulties. Every academy was trying to move into all possible fields. But very soon, we realized that it was impossible, and that each academy must concentrate on something, and when we have a smaller republic—for example, the republic I represent is one of the smallest republics of the Soviet Union—we must

concentrate on a very limited number of scientific problems. (Of course, such a republic as, for example, Ukraine, which has 50 million population, can take a wider range of problems.)

When all the scientists realized this, and succeeded in concentrating on a small number of problems in each of the republics and when we succeeded in those, the work of coordination became much easier. It is done by the committee, which consists of the presidents of the academies of the different republics. This committee also pays special attention to the necessity that each academy of sciences, of each Republic, must have its own fields of work, a special face which is different from the face of other academies.

At the same time, this cooperation between the academies of different republics has brought the rapid development of sciences in the Republics of the Soviet Union, which were 50 years ago, in a very backward stage of economic development.

Now, the scientists of those republics are feeding the results, and sometimes very essential results, into the whole of Soviet science, and world science.

We all know that one of the most important problems now before us is the problem of cooperation with the scientists of the developing countries. I shall make some remarks on this, and then finish my contribution.

We should consider that the most important thing in planning help for the developing countries is to base the help on requirements. And these requirements can be formulated only by the people and by scientists of those countries. Without knowing what the requirements are, and what the most urgent situations are for them, some people say, "Oh, yes, if a country is underdeveloped, then for them the first problem is the problem of food."

But I do not speak about food. I speak about science—naturally, both about theoretical and applied science—and it is for the developing countries themselves to decide which way for the development of science is most suitable for them.

We can have conferences here and can speak about the requirements and the needs of developing countries, but the most important thing, I believe, is to find the concrete problems and concrete needs they have—to learn this from them, and not just give advice to them.

They can easily find, what priority list is best for them. You know, we live in a world where the different nations have different styles of life, different ways of life, and, therefore, the requirements for science in every country are different. They depend not only on the economic level, but also on the traditions of the given country, the history of the country, and many other things we do not know. Therefore, the best thing, always, is, that the people, the scientists and

the intellectuals of the developing country, formulate for themselves all the requirements they have.

The organization that I head, the International Council of Scientific Unions, is not part of the apparatus of the United Nations. It is nongovernmental. The problem of environment is very broad. It has many and chiefly practical aspects, and at the same time, it is connected with many scientific problems. ICSU, which consists only of the unions in fundamental sciences, can take responsibility only for the scientific study of the problems which can arise, and already have arisen, connected with the environment problem.

Therefore, ICSU has a very large field of work. If we will succeed in this, we shall help the international organizations, such as in the United Nations. I imagine such a structure as the United Nations is considering all sides of the problem of the environment, and UNESCO will be responsible in the United Nations for the scientific problems.

However, ICSU, itself, in which all the disciplines are represented, can help very much in studying this. Here the first problems are connected with the methods of studying, the methods of monitoring, and of course, with evaluation of different factors. The scientific problems are of such large scale that they are quite sufficient for our scientists.

One more word: our Council of Scientific Unions consists chiefly, almost exclusively, of the unions in natural sciences and mathematics. Social sciences are not now considered. Therefore, we don't consi er ourselves very competent in social matters.

It will be wise, when we consider that we don't understand anything in politics, and yet I know that many scientists are very critical of politicians. But nobody has proved that the scientists can be better politicians than the real politicians themselves.

There is also another matter. We are an international organization. When you bring the practical consideration into this organization, then, of course, you destroy the very subtle, very delicate, but at the same time, very good and very sensible cooperation we have in the science.

Therefore, perhaps this is also another reason we prefer to be nonpolitical, but we see at the same time that it is impossible to be completely outside of the political. I will agree that it is impossible to be completely outside of politics, but we shall not rush very swiftly into politics. We should try to contact the social sciences, in order to widen our scope a little. But we must be very careful in all these things.

I have mentioned that ICSU is a union of unions in fundamental sciences, but of course we are concerned also with applications. For example, our Union of Pure and Applied Chemistry is not only interested in the purely theoretical work but in applied, also.

Recently, we have adopted, as a member of the ICSU, the Union on the Science of Nutrition. But, sometimes, many problems in the field of technology remain outside of our concept. But I think, since now the difference between the fundamental and the applied sciences is washed out in many fields. Therefore, many unions of applied sciences are inclined to ask us to accept them as member unions.

But of course our main aim is to consider the situations and to work on the situations which require international zeal and cooperation. When any scientific or even practical problem can be solved by the national means, it is perhaps not of the first importance for us. It is of importance, but not of first importance.

Of first importance for us are the problems which we can't solve without cooperation between different countries, and especially ICSU, without the interdisciplinary cooperation which is also necessary.

SCIENCE, TECHNOLOGY, THE MILITARY AND ARMS CONTROL

FRANKLIN A. LONG[1]

One of the most spectacular developments during the past two or three decades has been the increase in sophistication, quantity and effectiveness of military technology, very much of it based on new developments in science. This paper will deal with some of the consequences of this development and will concern itself particularly with ways in which science and technology can contribute to the effective control of this burgeoning military technology.

There is, of course, nothing new in nations striving for the latest and best in military technology. Indeed, it has frequently been argued that over the centuries the spur of military needs has been one of the most potent accelerators in the development of civilian technology of many varieties. In this connection it is sobering to remember that that august and honorific body, the National Academy of Sciences, was founded during the days of the Civil War, with the strong expectation that it would contribute to needed military technology. Even later, during World War I, the NAS affiliate, the National Research Council, was founded with much the same objective in mind. Again at the start of World War II the scientists and engineers of the United States were mobilized for war work through the then new Office of Scientific Research and Development.

Furthermore, there is nothing surprising in the fact that as the nations of the world become more industrialized and dependent on technology in their affairs, so will their military forces. Technology and industrial production are, obviously, interactive systems; products and processes which have been developed with civilian uses in mind frequently turn out to be of military consequence. Thus, new developments in civilian communications systems will quickly find their use in military communications. Assemblyline methods for producing civilian automobiles can be modified to the production of military aircraft. As the world becomes more technology dependent so will the military.

The last few decades, however, have seen the onset of two trends of such significance to military technology that they can be properly called new. The first of these is the close interplay between fundamental

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science and technology with the consequence that the rapidity with which scientific discoveries can be translated into technology has greatly accelerated. This is true in all areas, but notably so in the military. The second important trend is the development of major military research and development efforts, specifically focused on accelerated development of military technology. Indeed, the magnitude of this effort is now so large that substantial units of industry in the United States and in other developed nations are devoted almost exclusively to the development and eventual production of military technology.

In considering the impact of science-based technology on the military, it is common to think immediately of the strategic nuclear systems and with good reason. Two authentic revolutions in military technology have gone into the development of our current overwhelmingly lethal strategic nuclear forces. The first revolution was of course the Atom Bomb, which resulted in an increase in some orders of magnitude of the explosive kill-power per unit weight. An illustration of our terrible progress is to recall that the phrase "blockbuster" referred in World War II to bombs with one or two tons of TNT, whereas the comparible blockbuster now will carry megatons of TNT equivalent. The second revolution is embodied in the Intercontinental Ballistic Rocket which has given us the power to deliver these nuclear explosives to far distant places with high accuracy and with a thousand-fold decrease in time of delivery.

The terrible consequence of these two revolutions is that we have now reached the position where only a single decision and about thirty minutes of time stand in the way of the almost total destruction of any single nation in the world.

These fearsome developments in strategic nuclear weapons systems should not, however, distract us from the realization that the revolution in military technology has affected the entire spectrum of military affairs. Solid state devices and space satellite systems have revolutionized military communications. Computer based command, control and guidance systems have revolutionized the war of the battlefield, the so-called conventional war. And waiting in the wings are still newer systems based on the most recent technology.

A chilling exposition of the potential for new military weapons systems is to be found in Nigel Calder's recent book, *Unless Peace Comes*. The subtitle of this book is *A Scientific Forecast of New Weapons* and the Book itself lives up to its dread promise. Would you want to know about robot warfare, about militarized oceans, about environmental warfare? If so, this is the book for you.

Now the development of all of this impressive military technology, both present and future, does not come automatically. Indeed, the price that we pay for it is high in terms of both dollars and effort.

In the United States something between 10 and 15 per cent of the total military budget, depending upon the precise definition, is devoted to military research and development. Since the worldwide cost of military activity is about $200 billion/year, the implication is that something between $20 and $30 billion/year is involved world-wide in military research and development.

In fact, this figure may be considerably too low. In a speech last March, Dr. John Foster, Director, Defense Research and Engineering in the Pentagon, attributed an annual expenditure of $13 to 14 billion/year for military-related R & D for the United States and gave as his estimate a figure of $16 to $17 billion for the Soviet Union. Hence, estimating roughly the costs for the other nations involved, the world total may be closer to $40 billion than $30 billion. And this, bear in mind, is only for the R & D effort.

Furthermore, unless there are some exceedingly effective countervailing pressures, one can predict that the intensity of effort on military technology will increase. There are a number of reasons to expect this. One is that there now exist in all of the developed countries of the world trained, committed, and dedicated specialists in military technology. Consider, for example, the complex of trained people which exists in the laboratories and arsenals of the military services of the United States and in the aerospace and related industries whose principal function over the years has been to serve the military. This entire knowledgeable, technically-oriented and sophisticated apparatus exists fundamentally for the development of military technology.

A second pressure toward increased military R & D effort is the steadily increasing numbers of research opportunities within the field of technology. For the past number of years the world's scientific activity has been doubling approximately every 10 years. A comparable doubling time must entail for applied science and engineering also. The direct consequence is that there are ever-widening opportunities for the development of new technology, including military technology in particular. Several years ago, then Secretary of Defense McNamara spoke eloquently of the "mad momentum" of the arms race between the Soviet Union and the United States. The identical phrase is surely an appropriate description of the pressures toward the development of more military technology.

All of this intensive military effort is done in the name of national security. It by no means follows, however, that increased national security is a consequence of these technical developments. Indeed, it is a sad and ironic fact that as the United States has steadily increased its military expenditures and developed its impressive new military technology, its national security has steadily *decreased*.

No longer do we have any serious hope or expectation of successfully defending our country against nuclear attack, in the meaning of the word defense. Our best hope is only that our mutual nuclear deterrence will remain effective and deterrence is not defense. Mutual deterrence only promises that if deterrence fails there will be vast mutual destruction, destruction in other words, of attacker and attacked alike. And regrettably in recent months there is increasing evidence that even this limited concept of mutual deterrence is losing some of its credibility.

An excellent example of how a technically attractive piece of military technology developed in the name of national security, can nevertheless carry us down the path of diminished security, is the case of MIRV. This acronym, as almost everybody now knows, stands for Multiple Independently-Targeted Reentry Vehicles. The background to the development of MIRV and the reasons for concern about development have been many times outlined by such people as George Rathjens, Robert Klieman, and Herbert Scoville. A particularly fine summary of the situation is to be found in Herbert York's book.

Very briefly, the situation is this. It is now possible to replace the single warhead carried by a strategic nuclear rocket, as for example, the U.S. Minuteman or the Soviet SS-9, with a warhead which contains a number of independently-targeted nuclear weapons. The megatonnage of each of these separate weapons will, of course, be considerably smaller than the megatonnage of the single bomb which they replace. Thus, according to newspaper reports, the roughly one-megaton weapon of the current Minuteman rocket will be replaced in Minuteman III by a group of three MIRVs, each with only two-tenths megaton yield. Remember, however, that the bombs which devastated Hiroshima and Nagasaki were under 20 kilotons, i.e. 10 times smaller than these. Remembering also that, due to the cube root relation between damage radius and megatonnage, three properly dispersed smaller nuclear bombs can do more damage to a large city than the equivalent single large bomb, it is clear that the destruction carried by a MIRVed weapon is still enough to greatly devastate a modern city.

What are the military reasons for developing and deploying MIRV? A first reason is that it is an obvious response to an antiballistic missile defense system in that by launching a MIRVed missile more targets are presented to the defense system with consequently more opportunity for the attacker to penetrate through the defense or to exhaust the defense. A second reason which is sometimes advanced is that the greater total number of warheads which MIRV produces

permits the attack of many more military targets in the opponent's country.

The third military opportunity and the one which turns out to be the joker in the deck, is that if these separate warheads have sufficiently high accuracy, they again make it conceivable to consider the possibility of a nuclear first-strike, something which mutual deterrence had presumably made inconceivable. The arithmetic here is simple. Supposing two opponents each have identical numbers of land-based, nuclear-armed, intercontinental rockets emplaced in silos. Suppose each rocket warhead contains half a dozen MIRV's. Suppose, finally, that the accuracy and reliability of each MIRV is such that the targeting of three MIRV's on each of the opponents' launchers will give a 98 percent probability of destroying them. Then one country could launch a first-strike on the opponent's land-based launchers and airfields with half or two thirds of its force and expect virtually to destroy them. The country which delivered the first-strike would then be left with a large remaining nuclear force to use as a military threat to defeat the now disarmed opponent.

Actually, it is exceedingly improbable that a nation would find this scenario of attack at all persuasive. First, the United States and the Soviet Union would have in reserve their submarine-based nuclear missiles, and could use these for retaliation. Secondly, even the few land-base missiles and strategic bombers which survived this first devastating first strike might still be enough to wreak unacceptable damage on the initially attacking country.

Characteristically, however, military strategists are inclined to "worst case" analysis and hence even the remote possibility of a nuclear first-strike from a MIRVed system will almost surely accelerate the arms race. Thus one possible answer to a MIRVed force is a major defense system of anti-ballistic missiles. A second possible answer is to develop a force of mobile land-based nuclear missiles, i.e., rockets mounted on railroad flat cars or on automotive trucks. Still another conceivable response would be for the threatened countries to develop a strategy of "launch-on warning", i.e. a command and control procedure which could react so rapidly as to permit retaliatory missiles to be launched *before* the opponent's first-strike reaches its targets.

Any single one of these proposals may sound unpersuasive but *in toto* they are precisely the kind of concerns which diminish the credibility of a position of mutual nuclear deterrence, and which ultimately can lead to an acceleration of the arms race. And this is why I believe that the mutual deployment of MIRV forces will decrease the national security of both the United States and Soviet Russia. Specifically, I believe that a more rational analysis of national security would have

put the *mutual banning* of MIRV systems high on the agenda of the SALT talks and that, with some hard work and a little luck, this expensive and potentially destabilizing weapons development could have been avoided by both the U.S. and the U.S.S.R. I deeply hope that the opportunity to accomplish this has not yet vanished, but time is very short.

All of this brings me to the major thesis of this paper which is that it is of the greatest consequence for the nations of the world, in their search for national security to explore the alternatives to military systems and war, and to look specifically at the possibilities of arms control and disarmament along with peace-keeping programs for the maintenance of a different and more attractive kind of stability. More particularly, I want to develop the reasons for my conviction that, just as scientists and technologists have contributed so extensively to military technology and the arms race, so must scientists and technologists play a much larger role in the search for alternatives of greater security.

There is, of course, nothing new in the proposition that more emphasis is needed on the alternatives of peace-keeping and of arms control and disarmament. This is, after all, what the UN itself is all about. Arms control and disarmament is also the specific topic of the continuing efforts of the UN sponsored Conference of the Committee on Disarmament which meets in Geneva, Switzerland. Similarly, nuclear arms control is the whole point of the current SALT talks between the United States and the Soviet Union. The problem is that, looked at overall, these various efforts have been singularly ineffective. It is true that the disarmament negotiations have yielded a few positive results, e.g., a partial nuclear test-ban and a treaty on nuclear non-proliferation. However, the talks have not achieved any limitations on the numbers of and character of the nuclear weapons which the super-powers have and have been ineffective in obtaining significant limitations on conventional armaments. Similarly, the United Nations have had only very limited success in developing any truly viable alternative to armed conflict for the settlement of disputes among nations. In the meantime, the world arms race mounts steadily and new and more deadly weapons continue to be developed, deployed and in far too many cases, utilized.

A principal and primary need is for a very much greater understanding of the modern military systems and of the political and social impacts of a continuation of the military arms race. Equally, for world security there must be much more study and consideration of the alternatives to the military path.

The heart of the matter, it seems to me, is to develop a much deeper appreciation of the concept of *relative risks*. Somehow, we must develop an understanding that the current path of arms race and emphasis on

military systems for national security is in itself a terribly expensive, hazardous, and risky path. Once this fact is clearly understood, one can reasonably hope to go on and look in a more objective and thoughtful way at alternatives which minimize the centrality of military forces.

Two further points must be made. The problem we deal with is truly an international one, even though it involves the separately analyzed national security desires of nations of many sizes, types, and degrees of development. Hence the understandings of risks and alternatives must be themselves international. The second point is that because so many of the major present and future military systems are deeply based on modern technology, it is important that this technology be understood and considered in the evaluation of relative risks. Finally, it must be understood that if the various alternatives to a military approach are to be recognized and to be ultimately persuasive, there must be a much broader program of analysis and publicity. The current position of military is both a traditional one and a deeply entrenched one. It is too easy for all of us, citizens and specialists alike, to think first of military solutions to foreign policy problems; it is much harder to turn our attention to an analysis of comparative risks and gains. Furthermore, the momentum of technology toward new military systems is strong indeed. To counter these will take real effort.

What specifically can scientists and engineers do? As a first item, they can play an explanatory and tutorial role in their own country. One need only recall the ABM debates of two years ago to appreciate how terribly important even a moderate understanding of the scientific and technical possibilities can be to what is an essentially political decision. Similarly, one need only remember the enormous impact of Academician Sakharov's activities to appreciate what the role of one concerned scientist can be in a country like the Soviet Union. But beyond this internal role the scientists and engineers with knowledge on these military matters have a most important role to play at an international level and it is this I want to particularly speak of.

In almost all phases of these complex problems of international peace and security, it is essential that the nations concerned have clear common understandings of problems and of opportunities. As only one recent example, it has been of the greatest importance that the Soviet Union and the United States have developed in recent years a similar appreciation of the concept of strategic nuclear deterrence. Fortunately that understanding does now exist and is in turn an essential component of the effort to obtain strategic arms limitations which is now going on within SALT. Similarly, if we are to obtain international control over deployment of MIRV systems or of anti-ballistic missile systems, there must be an international appreciation of the military technology involved and of the consequences of deployment of these systems. And finally, because it is characteristic

of the current world that new military technology is steadily being developed, we must have procedures which give to the international community mechanisms for *continuing* analysis of these security problems. Fortunately, some mechanisms do exist and other possible avenues are available for further expansion.

Any discussion of these mechanisms must start with the United Nations. The UN is the sponsor and supporter of the continuing discussions within the CCD. The UN Assembly annually devotes considerable effort to discussions of problems of national security. And finally, there is within the UN a working study Committee on Disarmament which itself represents an avenue for increased international understanding. Each one of these different UN activities is important and each deserves expansion and strengthening. There exist, however, exceedingly important needs for international mechanisms which are less official and less formal than the UN. Fortunately, a few such exist. Let me tell you of two wherein the focus is heavily on the science and technology of these military problems. One of these is a highly informal nongovernmental group, officially called the Committee on Science and World Affairs, but more familiarly known as Pugwash. The second one is a Swedish-sponsored endeavor called the Stockholm International Peace Research Institute.

Pugwash was born from the Cold War and the realization of the terrifying characteristics of nuclear weapons. The initiating mechanism was a manifesto from a small group of physical scientists to the other scientists of the world urging them to come together to discuss these problems and to search for avenues for peace. Following this call, the first international conference of concerned scientists occurred in 1957, at the little village of Pugwash in Nova Scotia, hence the name. Since then the Pugwash movement has met steadily and has grown steadily larger and more influential.

A major achievement of Pugwash has turned out to be to give an *informal* and *unofficial* mechanism whereby scientists and engineers from the East and West, and specifically from the U.S. and the U.S.S.R., could meet to discuss problems of nuclear war and to search for solutions.

It is, of course, almost impossible to measure with any certainty the influence and accomplishments of an informal activity such as Pugwash. A minimum accomplishment has been the development of mutual understanding among small groups of Western and Eastern scientists about the character of these problems and of the need for alternative solutions to the military. Pugwash participants like to believe that their role has been a good deal more substantial. They are persuaded, for example, that their roles in the development of the nuclear test-ban and the non-proliferation treaties were sig-

nificant and that, perhaps, even the SALT talks were aided by earlier Pugwash discussions.

Let me give you a concrete example of the kind of thing an international informal organization like Pugwash can do. In June of 1970, there was held in the U.S. a Pugwash symposium on "Impact of New Technology on the Arms Race." Participants in this conference came from over a dozen countries and included five scientists from the Soviet Union. The Conference was a serious and professional one, with a number of thoughtful prepared papers. The results are just appearing in book form as a publication of the M.I.T. Press.

One of the important discussions at this conference concerned the probability that accuracy of intercontinental ballistic missiles will steadily improve with a genuine possibility that in only ten years or so the average miss distance may be down to less than a hundred yards. A clear implication of this possibility is that land-based intercontinental ballistic missiles may become obsolete. A second discussion at this Symposium focused on technical threats to the relative invulnerability of submarine ballistic missile systems of the Polaris type. One serious suggestion was that new and feasible developments in accoustic detection could greatly increase the vulnerability of these systems. This, if true, would be of major consequence to the maintenance of nuclear deterrence and clearly merits more thoughtful study and better international understandings. Pugwash studies of this variety will continue and will hopefully continue to be of international utility.

The second program which I wish to comment on where scientists and technologists play a role is the Stockholm International Peace Research Institute usually known by the acronym SIPRI. SIPRI is a relatively new organization, only about five years old. The initiative for its founding came from Madam Alva Myrdal, who became conscious of the need for more international studies during her assignment as Ambassador from Sweden to the UN Disarmament Conference in Geneva. She and her colleagues realized that a truly international study body was desirable and that it was also important to have a support mechanism which was comparatively independent from the major East-West powers; hence SIPRI. The substantial influence of SIPRI in its short life has amply justified the Swedish initiative. The SIPRI studies on seismic detection of underground nuclear explosions and on chemical and biological warfare are classics. The two yearbooks put out by SIPRI, one for 1969 and one for 1970, have also had a substantial impact on the understanding of the world's military activities and on analyses of possibilities for significant arms control and disarmament.

It is a devastating comment on the paucity of international information on problems of military technology, military forces and arms

control possibilities, to say that already SIPRI has shown itself to be virtually indispensable. Let me, for example, quote three of the many reviews of the first SIPRI yearbook, two from the United States and one from a communist country:

> If peace on earth depends upon the world's people having pertinent information made available to them, then SIPRI's Yearbook will be a significant step towards peace. [Los Angeles Times (USA)]
>
> The Yearbook pulls together mountains of material, previously known in detail to specialists but unavailable to the general public. It conveys a dizzying sense of an arms race spinning out of control. [The Washington Post (USA)]
>
> This study represents probably the most complete analysis ever made of world armaments. [Borba (Yugoslavia)]

In an important sense, both the Pugwash activities and the SIPRI studies represent useful, significant, and expanding efforts in this important field. Their very existence is encouraging. So also is the fact that in many of the nationally supported study efforts on problems of peace and control of the military, the importance of international understanding is increasingly recognized. Looking only at recent progress, one could even be mildly encouraged. If, however, one views this progress in terms of the massiveness of the problem itself, for example, in terms of the two hundred billions dollars per year spent on military systems and the continuing momentum toward new and more deadly military technology, it is also possible to be profoundly pessismistic.

Speaking more positively, one cannot help but feel that there simply must be much more effort by both governmental groups and non-governmental groups, devoted to studying the problem of national security from a much broader position than simply the analysis of military programs. Furthermore, these efforts should be at both the national level and at the international level.

One area of non-governmental initiative which is particularly hopeful for expansion into international programs is the formation of Societies for the Support of Peace Studies. Many countries, and this includes the United States, are in the position that the civilian groups which support studies on alternatives to the military are too small and too fragmented. Unhappily, one even occasionally senses competition between the various small groups. How much better off the United States would be if there were one, vigorous, well-funded Society for the Promotion of Peace Studies? As I envisage it, such a Society could, in the first instance, be founded by the concerted efforts of the central U.S. scholarly societies as, for example, the National Academy of Sciences and the American Council of Learned Societies. This

sponsorship could insure a seriousness of purpose and dedication to objective studies which would be of the greatest importance. Such sponsorship would also help in obtaining the very substantial amount of funds that would be needed for an adequate number of serious studies. The existence of such a society could galvanize efforts in this country and could be the focal point for a whole spectrum of non-governmental activities. I very much hope that such a society will come into existence and soon.

But even if one has vigorous non-governmental programs, there still remains a most essential need for serious studies within national governments themselves. The United States Congress can justifiably be proud of what it has done in this field, but it remains true that substantially more can and probably should be done.

Looking at present activities of the U.S. Congress, the very existence of this Panel and its predecessors sponsored by the House Committee on Science and Astronautics shows a Congressional recognition of the responsibilities of scientists and engineers to fields beyond their own specialties. The increased study of military programs which Congress has carried out within its normal committee structure is a most heartening direction. Finally, the existence of the small and informal, but nevertheless very important bi-partisan effort called Members of Congress for Peace Through Law is a clear recognition that something more is needed than just a study of the military.

What else might Congress do? As one very tentative suggestion from a concerned citizen, I would like to urge consideration of a new and major joint House-Senate Committee explicitly committed to broader studies in these areas. My specific suggestion would be the development of a Joint Congressional Committee for National Security. I would hope that the interpretation of national security by the new Committee would be broad enough to include a study of non-military alternatives as well as of military programs and to include a study of the impact of internal U.S. programs as well as of international needs and goals. Speaking even more generally, the new Committee could be the place where Congress would focus its studies of relative risks, and it is these studies which I think are critical to a true understanding of national security.

In making this suggestion to Congress I am very much aware that there already exist congressional committees whose domains cover important pieces of the problem that I am addressing. However, there is real need for a single forum within the Congress for a truly comprehensive and integrative analysis and debate on matters of national security including not only the foreign affairs and military aspects, but also those aspects of domestic policy which in fact affect our national security. It is interesting to note that within the executive

branch of our Government the integration which I seek for Congress has already occurred. Within the President's office there exists the National Security Council whose purpose is to take the broad, integrative view.

What I am urging is a congressional organization which is the counterpart of the National Security Council in the sense of embracing as wide a spectrum of considerations as is embraced by the NSC. Congress already has joint congressional committees responding to other broad national needs. As far as I am concerned, the need in the national security area is no less, and indeed I would argue far greater than in these other areas. In the committee as actually organized, I would envisage much of the membership drawn from the standing committees of the Congress having legislative responsibility for the component areas of defense and foreign affairs as well as domestic matters. What I am proposing, therefore, is a very modest step in the direction of the congressional committees which are organized along functional rather than departmental lines. I take some comfort from the fact that, in this respect, my proposal is in accord with the results of nearly every major study on reorganization of the Federal Government.

NATIONAL SCIENCE POLICY—PRELUDE TO GLOBAL COOPERATION

EMILIO Q. DADDARIO [1]

The theme for this year's panel meeting—International Science Policy—seems to me to be an especially appropriate one for several reasons. First, it provides a focus for much of the work and discussion that has resulted from previous annual meetings of this Panel on Science and Technology. These meetings have dealt with such issues as basic science, applied research and the world economy, information management, and international policy. This year's session embraces all of these themes in an attempt to focus on the central question of how science and technology can best be employed for the benefit of all mankind, not just Americans, Europeans, Russians or Japanese.

Second, it offers us the opportunity to consider new approaches and mechanisms for broader international scientific and political cooperation.

Third, it serves to remind us of our narrow parochialism and of the need to integrate more completely our own national science activities with those of other nations. In addition, this theme is appropriate because of its timeliness. We are, I believe, at a pivotal point in the history of man and in the relations among nations.

Shaping a new framework

The fact is that, today, the old framework of international politics is no longer compatible with reality. That framework was predicated upon conditions and attitudes that have given way to change. It is a framework characterized by spheres of influence, military alliances between nation-states, and the divisions of national sovereignty. This is not to say that the framework is inoperative or non-essential today. To the contrary, this latter aspect—sovereignty—has become one of mankind's major religions. It has become as historian Arnold Toynbee has written, "A religion whose god is a Moloch to whom parents are willing to make human sacrifices of their sons and of themselves and of all their fellow human beings too, if a "conventional" war should escalate into a nu clear one."

However, it has become evident that the concept of sovereignty and the traditional means of conducting relations between nations are no

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longer sufficient. The reason for this is that technology has largely changed the world, and in doing so it has rendered the old framework very vulnerable. Today, we are witnessing an increasingly rapid compression of both time and space. For example, despite the extraordinary capabilities of almost instantaneous world-wide communications and their promise of bringing the peoples of the world closer together, we see today continual social disintegration and upheaval. This has led to the contemporary paradox whereby the human race is simultaneously becoming more unified and more fragmented. We now seem to have a dichotomy on our hands—either lasting cooperation or complete political dissolution—the potential for either being greater than in any previous period in human history.

What is required is a new framework. Indeed, perhaps one is already emerging in terms of global politics and in the direction of a new ecumenism. And yet, while the concept of world government is not new, it is still far from being realized. Traditional political habits and the institution of national sovereignty are especially strong and obdurate to change.

What I believe to be a more realistic assessment for the foreseeable future, is much broader global cooperation among sovereign nation-states. This, I believe is not only possible, but mandatory. For today, we possess the technological and organizational capabilities to begin not only to manage human affairs on a worldwide scale, but to feed, clothe, educate and otherwise care for all those who wish it. On the economic plane, we have already begun go see evidence of this kind of capability in the form of large-scale private multinational corporations whose activities span many different sovereign and market boundaries. I believe that we could begin to do the same on the political plane if man would only express his will to do so. The very existence of such corporate activities shows some progress in this direction. However, their continued existence largely depends upon the progress we make on the political plane.

Purpose and priorities

It must be recognized, however, that before there will be any real global cooperation, there must be far greater consensus on its purposes. What are these? Is it to enhance material well-being and intellectual development? Is it economic growth or a massive education effort? Is it limited arms control or an international peacekeeping mechanism? Is it expanded medical health care or more adequate housing?

And what are the priorities? How do we develop a consensus in this area? How do we reach some balance between near-term localized problems and long-term global problems? Let me take just one example: a familiar one, but one that concerns me greatly.

I refer to the exploitation of our natural resources. We are all aware of the statistics concerning the consumption rate of these resources

by the industrialized world. We are also awake to the political sensitivities of resource exploitation in the developing countries. And, of course, we are all told that the earth's resources are finite. And yet, most of us in the developed world are still unwilling to restrict our own activities, to restrain our voracious appetites to consume, and to contemplate seriously the long-term implications of present-day actions. We prefer, rather, to go it alone as nations, exploiting and competing for those resources. We favor the near-term material benefit to the potential long-term loss, with only minor consideration of the real and present social costs, both to international political stability and to the developing countries themselves. The point is, we have an opportunity here to develop new global mechanisms that will not only give nature a helping hand, but will diminish the social and political tensions that are mounting in the developing world.

In one sense, however, there appears to be at least one true consensus among most major countries. They now accept the principle that they should aid developing countries. There are at least four underlying beliefs supporting this principle. First is security. The realization that the dichotomy between the "haves" and the "have-nots" breeds serious global tensions is well understood. Second, there is the belief, to which I have just referred, that resource exploitation in the developing countries creates interdependencies with the developed world. Third, there is the recognition of global problems—such as those relating to the environment—that will require new cooperative arrangements and, perhaps, international standards that must be administered by both the developed and developing countries. And finally, there is a certain amount of altruism on the part of the developed nations who share a concern for the health, well-being, and aspirations of those in the developing nations.

This principle is recognized and shared by the developing nations with *both* a sense of mission and a sense of guilt. Eugene Black, former president of the World Bank, noted recently:

> We talk about the existence of a coherent "Third World" but in reality it only exists in our own minds, in the minds of us in the rich countries, particularly, whose culture bequeaths to us both a sense of mission and a sense of guilt in world affairs. Confident predictions, then, about the future of the "Third World" will more often than not appear as acts of aggression in the eyes of those who are so summarily lumped together.

Some distinctions

Part of the problem that I have alluded to stems from our own cultural bias, "a bias that grows out of our own experience, not from that of the poor countries." It is not so much that we are determined to

force our own value system upon the developing countries, but that we often fail to understand their value preferences, which may be foreign or inimical to us.

For example, when we routinely apply the handy measuring device —the gross national product—to compare countries of differing cultures, we often startle ourselves with the results. The point is, the application of the GNP measurement to two economies, such as the United States and Colombia, of remarkably different bases of production can often seriously exaggerate the real differences in welfare between those two nations.

Similarly, on a somewhat less abstract level, the pervading public concern over environmental quality, now largely shared by the developed countries, is not clearly felt in many poor countries. Indeed, they would probably be glad to import our environmental problems if it meant a decidely higher standard of living for themselves. The problem is not one of tying our assistance with concrete value clauses or restrictions, but of trying to understand better these differences and, through mutual consent, to make modifications where feasible.

This is difficult. It is especially difficult for a nation such as ours which has experienced so much of what is good and what is bad in the process of its own growth. John Locke wrote over 250 years ago in his *Second Treatise on Government*, ". . . in the beginning, all the world was America." In this sense, all the world is America in that America is probably the first to experience the social, psychological, political, and ideological dilemmas produced by its own accelerating scientific and technological development. All this is not to say that the situation precludes or should inhibit our imparting the knowledge gained from our own experiences to those nations in the developing world. Rather it is all the more reason—to re-emphasize my point—why we in the developed countries should be more sensitive than we have been in the past to the different needs, wants, and aspirations of the individual developing countries as we consider new approaches to global cooperation.

Reappraisal of science: A catharsis

Let me now turn to my major area of concern: the development of an international science policy framework. In the same vein as Locke, it is evident today that America has become more candidly critical and demanding of itself than many countries. One has only to witness the continual flow of reports, commissioned studies, and investigations to see the reflection of this introspective and deliberately sober mood. I believe that this mood of self appraisal is indicative of both the deep dissatisfaction with our reluctance to cope with change and with our realization that, when confronted with change some of our traditional precepts and values have been found wanting.

No contemporary area of effort more exemplifies this mood than that of science and technology. To be sure, today's catharsis in science has been building steadily for over a decade. To no small degree, the present public disenchantment with science and technology is partly of the science community's own making. It stems from one serious misconception and two ancillary—but by no means unimportant—failures.

First, the misconception has been that science, following World War II, was immediately and immaculately enshrined by the general public as a new and permanent 20th century value, inviolate, and, like Caesar's wife, above reproach. It is true that everyone had been impressed by the enormously important contributions of science and the academicians to the war effort. However, there was also widespread concern—especially within the scientific community itself—over that which science has wrought. Nonetheless, the misconception began to spread that support of science qua science was a thoroughly accepted doctrine. This view was further enhanced by the expenditure of increasingly generous amounts of Federal funds. The trouble with all of this was that the general public—and the government too—really had quite different and more near-term goals in mind than just research for research's sake. It has come as no slight shock to some in the science community to discover this fact.

Second, what developed during that period was a form of laissez-faire attitude with regard to the conduct of research, which, I must admit, still exists in some quarters of the scientific community. This attitude has been characterized by one science critic as echoing the 19th-century businessman's individualist ideology:

> A hundred years ago the laissez-faire ideology may have been adequate for the needs of both scientists and society. Today, however, the arguments for unhampered science are as irrelevant as the arguments for free private enterprise by mammouth corporations, or the arguments for an unregulated press by the mass-media monopolies.

Science and scientists, themselves, must be more responsive to the needs of the society which through its support makes their work possible. To fail in this or to perpetuate the attitude that science be immune from the social sphere is to repudiate the source of its strength and future growth.

Thirdly, I believe the general public believes that the scientific community has failed adequately to insure that research results are made available in a timely fashion to meet society's broader social needs. As serious as this charge is, I think the scientific community should take some heart from it. For the implication of the criticism is that scientists have the capability to do far more than they have done

in the past in helping to resolve many of our contemporary national domestic problems. To be effective, however, the scientific community is going to have to look inward upon itself, more so than it has in the past. One of the greater potentials lies in the growth of multidisciplinary research capabilities, which the National Science Foundation has been nurturing in recent years. The dual thrusts of this effort—breaking down the traditional disciplinary barriers and developing new mechanisms for the timely transfer of research results into applications—hold promise for bringing scientific research into greater service to society.

I offer these criticism not for rhetorical effect, nor as the parting shot of one who is released from the burden of public office. This would be impossible for son eone who has been as closely associated and concerned with these issues as I have been for over a decade. I intend them as helpful warnings. Moreover, in making these criticisms, I do not imply that industry, the general public or government are absolved of blame. Far from it.

Industry—with its preoccupation with the immediacy of return on its investments in science and technology, its lack of product assessment capabilities, and, up until quite recently, its marginal concern for the physical and social environment—surely shares some of the blame. The general public's fascination with big technology, its lack of true understanding of both the scientific process and the importance of fundamental research, and its sometimes fickle nature have contributed greatly to the present state of affairs in science and technology. Finally, the government's own fractionated organization for science and technology, the lack of stable funding mechanisms, and its bureaucratic inertia to change are well known, serious faults.

All of these weaknesses point to a central unresolved question of our times—how can the pursuit of science and technology best serve man? It is because of this still unresolved question that I personally feel so strongly about the need to formulate national science policy. Such a policy would provide the goals, priorities, and needed direction for the nation's scientific enterprise in the last quarter of this century.

How would such a policy relate to what has been called "International Science Policy," and what does the latter concept mean? To me, international science policy is not synonymous with "global" science policy. Neither does it mean a unified supra-national doctrine.

Rather, I view the concept more in terms of an "incorporation" of policies, doctrines, agreements, and other cooperative arrangements between many nations. But, it is more than just an amorphous assemblage. It implies to me in the fullest sense a synthesis of goals and aspirations of all nations of the real and potential contributions of science and technology to our planet. As such it could provide a new framework within which global cooperation might flourish.

Problems and prospects for global cooperation

Let us look, then, at the problems and prospects for broader global scientific cooperation. And, I would like to return, first, to my earlier comments concerning the developing world.

Dr. Jorge Sabato, technology manager for Argentina's National Commission for Atomic Energy, posed some interesting ideas in a paper presented before this Panel three years ago. In that paper, Sabato distinguished between the undeveloped and developing countries, and listed several stages of scientific development which I believe are pertinent.

These key stages of scientific and technological development are as follows: first, the strengthening of a country's scientific and technical infrastructure; second, the creation of a social consensus for the importance of science and technology; and third, the inventment stage in which scientific and technical innovation is coupled with and introduced into the productive process of the country.

These stages are clearly interrelated and there is some necessary overlap and feedback in the various stages. Moreover, because reality is heterogeneous and dynamic, we must consider these stages only as general guidelines, yet be aware of the unique differences between the various undeveloped and developing countries. Clearly the undeveloped countries are far behind in terms of their scientific and technological development. This is all the more reason why the resources and assistance of both the developing and developed countries can and should have a tremendous positive impact. Cooperation, then, with the undeveloped world would take on more of a missionary role—and I use that phrase advisedly—than might be true with the developing countries.

Global cooperation between the developed and developing countries holds, perhaps, even greater promise for rapid economic and social development. The very existence of even the most rudimentary scientific and technological infrastructure can have a decided "multiplier effect" in that it represents a critical base that is often lacking in the undeveloped countries.

Turning to the question of global cooperation among developed countries, this is an area that is most promising and still very problematic. It is problematic because of the traditional instrusion of the question of sovereignty into the process of cooperation. Despite the history and wisdom of keeping science aloof from national conflict, there exist many subtle sovereign boundaries which inhibit global cooperation between developed countries. And yet, I think we are now seeing a diminution of these barriers. Take, for example, the recent French public opinion poll in which 66 percent favored the emergence of a European government that would have decisive powers in such areas as scientific research as opposed to 15 percent which favored local French government role. Similarly, another recent poll in the United States indicated

that 75 percent of all 18- and 20-year-olds support joint exploration of space by the Soviet Union and the United States. It is interesting to note, that only 48 percent of those 50-year-olds-or-older support such a joint adventure. This is as true in scientific affairs as it is in economic relations and foreign aid. What I see, then, is the need to develop more fully the multilateral approach to scientific cooperation.

With respect to policy formulation, it should be recognized that the development of a national science policy may not necessarily be required, much less desirable, for each individual country. What I am suggesting is that within the developed nations it might be more fruitful for groups of nations—say, several Western European countries—to formulate a unified policy that would serve the goals of a specific community of nations. To follow the cliche-ridden pathway in which international status is measured in terms of such symbols as supermarkets and laundromats would be foolhardy. There is no reason why each nation should have its own unique science policy.

Consideration of this point brings to mind a new type of cooperative mechanism that might be worth pursuing. For lack of better words, I shall call it an International Science Policy Committee. I am using as my prototype the existing Development Assistance Committee (DAC) of the present Organization for Economic Cooperation and Development (OECD). For all its quietness and obscurity, this 16-member standing Committee has provided over the years an important coordinating and consultative function for the development efforts of the free-world donors. It has no power to dictate aid policies, but its credo and influence stem from the belief that consultation is in the interest of the members of the group. As such, it provides an important means for the exchange of information and positions on the substance and direction of the aid policies of its individual members.

I believe that a similar type of mechanism might be fruitful within the realm of national science policy. Such a continuing forum would permit the formal exchange of views and positions with regard to the pursuit of science and technology within the developed nations and in their relations with the developing world.

It might also provide the means through which broader collaborative scientific activities could be discussed and formulated.

In a world that is witnessing the increasing alienation of mankind, there is an increasingly great need to provide means for holding things together. In this respect I believe that global scientific cooperation holds an especially important promise. As historian Kenneth Clark has observed, "lack of confidence, more than anything else . . . kills a civilization. We can destroy ourselves by cynicism and disillusion, just as effectively as by bombs."

ADMINISTRATIVE REQUIREMENTS FOR ADVANCING INTERNATIONAL SCIENCE POLICY

JAMES E. WEBB [1]

This is my first meeting with this committee since my retirement as Administrator of the National Aeronautics and Space Administration. I would like to say first of all that, as we come to a time when in our own country and around the world many forces are at work which call for new policies and programs, and at a time when many of these fall in the specialized fields of science and astronautics, the fields of this committee, I think it is of great importance in this country and, indeed, throughout the world, that this committee has developed this panel method of feedback through informal exchange of views with a wide variety of knowledgeable persons, that the establishment of this as an annual procedure is a tribute to the zeal of the political leaders of our Nation in this field as well as to our scientific, technical and administrative leaders in seeking new and better ways to focus on the needs of the future.

This committee has developed, I believe, an improved method for the leaders of science and technology to provide the members of the committee their views and to come to understand how Congress proceeds with their work. These annual panel meetings provide one of the best means devised for Members of Congress to obtain early impressions of program yields from keen observers and to become aware of developing trends before they become major issues.

There is another value. This helps Members of Congress to evaluate much of the advice they receive. Here they can assess the ability of panel participants, many of whom are specialists, to understand many of the features and requirements of public policy which must underlie congressional processes and congressional actions.

Now, as all of you know, my own major interest is in practical ways through which policy can be made effective. It seems to me that we have been most effective when we have used forms of organization and administration, forms of policy evolution that brought the substance of scientific and technical projects into close working relationships with effective public management. I am happy that you have listed me as an officer of the National Academy of Public Administration.

I might say that it is important, I believe, not only to have an Academy of Sciences and an Academy of Engineering, but also an

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Academy of Public Administration, which in many ways is a necessary interface between scientific knowledge, engineering knowhow and that vast environment beyond this narrow community of scientists and the somewhat larger, but still narrow community of technologists and the broad arena of the environment in which these forces must do their work, must come to rest, must be evaluated if they are to make their excellent contributions.

I would like to say that these meetings also from my experience in the space agency help prepare for the committee a broadly based foundation of fundamental concepts under which its members can evaluate policies and on which executive departments and agencies can build the justifications for their annual authorization and budget requests.

We begin each year on the work done in the previous year and this panel method, I think, lays an important foundation for the work which comes specifically on the authorizations and budgets.

Now recently in this forum you have studied ways and means to advance toward a national science policy, and you have recommended a national commission to lay down the basic ingredients. Today you are moving on to the need for the establishment of an international science policy and in this area, also, you may find several ways to meet a need for clarification, perhaps more clarification than you can give in these meetings. Of course this clarification must run to the elements that would be desirable for incorporation in such a policy. If, indeed, you do wish to continue through some mechanism or means or further study, then perhaps you might think of the question of an international commission to parallel your national commission, or you might think of a national commission on international science policy. But it does seem to me that in any case and for any mechanism, it is important to recognize that others are at work in these fields.

For instance, recently the Committee on Foreign Affairs in the House of Representatives through its Committee on National Security Policy and Scientific Development, issued a report prepared by the Legislative Reference Service of the Library of Congress entitled "Toward a New Diplomacy in a Scientific Age."

This report concludes—

> Science and technology have effected changes in the substantive tasks of foreign policy, in the methodology of diplomacy, in the management of information on which diplomacy is based, in the intellectual training of diplomats, in the range of present options of negotiations, and in the prospects of future evolution of diplomacy, foreign policy objectives and the international political system.

This report also specified eight issues that have been chosen for further study, as follows:

(a) Evolution of International Technology.
(b) World of medicine.
(c) The brain drain problem.
(d) Food and population.
(e) Scientific diplomats.
(f) Generalists and technical sophistication.
(g) World scientific community.
(h) Computers and proliferating information.

In addition to these eight issues chosen for further study, the Committee has proposed or this report has proposed an interesting methodology for the selection of further issues for study, the basis being:

First, The subject should be of substantial moment and be regarded as such.

Second, The subject should have a significant technical content, so that it involves a problem of communication between the expert in the field and the generalist concerned with the diplomatic implications.

Third, The problem should involve some aspect of "Science in Policy" or "Policy in Science," that is, it should deal with the application of science and technology to advance some international policy of the United States, or it should deal with some way in which U.S. science and technology is sought to be strengthened by diplomatic action.

Fourth, The subject should have had sufficient continuity and persistence as a problem before the diplomatic community to enable observation of changes that have occurred as a result of national action.

I refer to this report by this sister committee as only one indication, which I am sure is welcome to the chairman of this committee, that just as this committee is reaching out for a better understanding of the international opportunities inherent in science, so is the Committee on Foreign Affairs reaching out to better understand the implications of science for diplomacy.

Now, perhaps the identification of international science policy and practical ways to make it effective might be pursued further and then maybe some form of joint effort of these two committees might be worth consideration. In any event, this action by the Foreign Affairs Committee shows the growing recognition in Congress, and elsewhere, that no area of international concern can be oblivious to the need for a further strengthening of the scientific underpinning on which so much of modern life rests; and it seems to me that one essential ingredient of an international science policy is how we can learn from our experience to enable leaders of many nations to work within their own

nations to improve their own scientific competence and relate it to engineering development that follows it, relate it to public policy, project approval, allocation of resources.

This committee, and I hope this Panel, are well aware that whether science policy is to cover national relationships only or is to be brought into the international field, it must include a proper perspective on developments in the fields of aeronautics and space. The use of the air is just beginning.

The technology of rocketry added to that of aeronautics has opened up vast new areas for geographic and scientific studies, and at the same time it has provided transportation systems that can deliver cataclysmic destruction to any destination on earth or in the near solar system. We all know that rockets now regularly deliver to their chosen destinations meteorological satellites that give the everyday user of weather information, as well as the meteorologist-researcher, what he needs in accurate and dependable form. Rocketry now provides mankind with the synchronous communications satellite relay, with the early promise of economical access to men, computers, and data banks at any point on earth from any other.

Rocketry will soon place multispectral sensors linked to preplanned computer programs in positions above the earth which will permit an overview of large regions. Many features of all activity carried out by man will be put into perspective. Worldwide inventories of resources in such fields as agriculture and energy will become possible. The technology for handling massive amounts of telemetered data in real time can soon give the generalistic human mind a wide-angle panorama of the major features of the complex situation which exists in nature, and, in addition, can permit the specialist to penetrate down into the data and isolate and study in depth one element of the complexity. In these and many other ways, rocketry is laying down basic challenges to the older concepts which have guided national and international policies. An international science policy must certainly help nourish an environment in which such powerful new tools can be brought to the service of all nations ready and willing to cooperate in their development and use and we all know that this means a larger science effort in many nations.

It is seldom clearly stated that the engineer, working toward the creation of a most useful machine from applying scientific knowledge, makes possible gains at a given point in time from contacts with research scientists, who can clarify for him where the forward-moving front of knowledge is at that given time. He also needs these contacts, and help of scientists, in applying the scientific method to his developmental problems. So it would seem to me that one important ingredient we should seek in international science policy would be to make clear to national decisionmakers this value to engi-

neers which can come from added emphasis on science. The advice of research scientists on their most difficult developmental problems should be make clear to those who allocate the resources. An international science policy could emphasize this.

It seems to me that we have not made that sufficiently clear in this nation, and that is one reason we have found less public support with time.

In this space age there can be little doubt that sovereignty is no longer as absolute a quality as it once was. Powerful new international cooperative systems challenge such older concepts. Decisions by nations or groups of nations to enter or remain outside these systems can no longer be safely based on short-term desires. They must also take into account long-term consequences.

New technology has already begun to build into rocket systems some types of controlled or flexible use characteristics which offer many new possibilities. In some ways the qualities we have attributed to the airplane are being built into rocket systems. Some of the limitations we have previously associated with rocket systems will not apply in the future. A continuation of the progress we have made in the past decade will undoubtedly make possible the incorporation of space components as integral parts of complex and very efficient global systems. These systems will also use ocean and air components. Although we cannot see clearly today the full advantages of such ocean-air-space global systems, it is already clear that the incorporation of a space component adds for the first time an unlimited element and thus greatly extends the previously limited possibilities of all the other elements. Thus, the economic justification for investments in such systems will now have to take account of this unlimited feature which can vastly expand the utility of the whole system. In these investment decisions, some certainly in international ventures—those who have to make these investment decisions can gain a great deal from contact with nationals in their own country who can give them adequate and proper advice as to the limits to which current knowledge permits reliable operation. There is nothing more difficult than international negotiations where some national leaders have to rely on competence derived from outside their own nation.

One important ingredient of an international science policy would be the commitment by all nations to an increasing competence in every nation available to its own national leaders as we work toward solutions to the great problems that Mr. Daddario has so clearly laid out here today.

Let me turn now to national policy and say that in many fields, these same new and emerging capabilities will add tremendous new complications. This is true in the national as well as the international field.

Truly, diplomats as well as scientists, and administrators as well as negotiators, must seek ways to work through these complications to reach the promise of the future. All possible efforts, it seems to me, should be given to building a framework in which the total of the disciplines, skills, knowledge and leadership qualities in every field can realistically recognize these complications and find better means to work together to overcome them. With the opening of space and with the development of systems to use its special advantages cooperatively with the advantages of the oceans and the atmosphere, the world emerges into a new era.

If through international negotiation a framework of policy can become a guide to all disciplines and the leaders in many nations, then it seems to me that we start with a better common basis and can work toward ways and means to overcome these complications and move toward the promises which these new areas of scientific knowledge and technology make possible, but only possible if there is an application of resources, if investment decisions are made, if political leaders can live with those investment decisions in their own nations, and, indeed, if each nation in the system derives practical, visible benefits.

In effect, our present knowledge as to what advanced industrial nations can do individually and collectively with rocket transport systems means that policies based on coercion and intimidation pose a greater threat than previously. On the other hand, policies looking toward international cooperation can be more productive than ever before.

Certainly, there can be no objection to the statement that science and technology are not separable from other forces in modern life. It is important to recognize that a decision to defer consumption and invest in education and research is difficult and requires a certain amount of confidence in the future, an expectation of at least a minimum level of stability. The same is true of any economic system where widely diffused decisionmaking is expected to accept long term risk for long term gain. International science policy cannot ignore this need for increasing those factors that build confidence, particularly in the less developed nations.

In addition, for a science policy to be effective as a part of an international code of conduct and to represent a common, widely-held aspiration, it must offer clear benefits for those nations which adopt and follow it. Some of these benefits must extend beyond those which a nation can expect from its own individual national science policy.

Let me quote two statements made by Jean-Jacques Salomon in an article in Minerva a few years ago entitled "International Scientific Policy":

The first quotation is as follows:

> Though there is no such thing as an international scientific policy, there is at least a system of international relations in the scientific sphere. This system is not immune to political stresses, and it is greatly affected by the activities of governments when they intervene in the projects institutions and investments upon which science depends.

And I think what we are seeing is a more persistent requirement for intervention in the decisions affecting the scientific community than was necessary in years past. Leaders and nations cannot stand aside from participation in many activities which for many years they paid little attention to.

The second quotation from Dr. Salomon is as follows:

> Experience has shown that governments will not undertake large-scale combined action and set up scientific organizations (or extend the competence of some existing organization to cover scientific questions) except when prompted by one or more of the four following motives—only the first of which is purely scientific:
>
> 1. The research is to be devoted to an essentially extra-national subject (meteorology, oceanography, etc.)
> 2. It requires expenditure which no country could meet from its own resources (nuclear research, space research, etc.).
> 3. The scientific activities in question are believed to contribute to some wider economic or military project for which the countries are pooling their efforts;
> 4. Participation in this form of scientific cooperation is likely to enhance or maintain the international prestige of the individual countries.

The events of recent years have not given evidence of a great change in these statements made in 1964. For those who believe that international cooperation can cure most of the ills of the world, however, if we could just find a way to bring it about, it is important to note the absence of any reference to cooperation for the sake of cooperation. If cooperation for the sake of cooperation is a desirable goal, and I am one who believes that it is, both for individuals and nations, and even a small area of cooperation gives the promise of enlargement of the advantage of all those cooperating, then it seems to me that the question arises as to whether we can incorporate this goal of small beginnings, expansion of cooperation for the benefits of cooperation, into more specific areas and activities. Can we join with that idea specific things which meet this criteria set by Mr. Salomon?

Have we learned enough about the conditions essential for advancement of science and the gathering of its benefits to propose a joint or

cooperative policy of a deliberate fostering of those conditions which produces scientific advancement by many nations within their own economic, political and social structures and patterns of life? Is there a common set of criteria by which the leaders of the nation can guide themselves in doing those things which they can reasonably expect to lead to scientific advance and without which they are not likely to get it? If so, perhaps this is an element for incorporation in an international science policy. The very fact that all have agreed on this basic requirement tends to increase the capacity for intimate discussion and acceptance with confidence that signatory nations will be guided by this policy.

Could a major international policy goal be to substantially increase throughout the world the amount of effort dedicated to scientific research in an area where every nation needs to know more than is known today?

Can we build on our experience in the International Geophysical Year and the International Year of the Quiet Sun so as to evolve an international policy related to further study of the sun under which all subscribing nations could justify national investments and the undertaking of a commitment to follow the agreed international policy though the level of sophistication was very different in each of the countries?

Could one goal of an international scientific policy be to increase the ability of all nations to use the scientific method itself in approaching their own national problems and thus increase, on an international basis, the total effect of the values to be derived from the scientific method?

Is it possible that under some form of international science policy, a steady increase in the competence of scientists in all nations could be made available to the political leaders of those nations so that each nation would have leaders and diplomats in a better position to negotiate those international arrangements most conducive to the achievement of its national objectives? Let me repeat, political leaders need the assurance that comes from confidence that they can rely on the knowledge of citizens of their own country rather than on those from other countries.

Is it possible to approach this problem of defining goals for an international science policy by assuming that for a particular nation the state of scientific inquiry and its relationships to education and to technology can be assessed and understood within the forces at work in the total of the economic, social and political system of that nation? If it is possible for an individual nation either on its own or in association with others to make this assessment, and then to seek practical and effective means to remove difficulties and make more compatible to scientific progress the environmental factors which are most directly

related to an increase in the rate of scientific advance, I would ask whether or not the sum of all this effort constitute in itself an international goal—the goal of substantially increasing throughout the world the total use of the rationale approaches of science and the development of technology from these activities.

Perhaps in a very practical vein, I could end by saying there are about four, maybe five, concepts which I believe can start at a relatively small level and offer the possibility of enlargement as men and nations work together within the framework of a policy.

The first concept is that of power together as against power over others. It seems to me that the United States is in a particularly good position to say to the world: "We have developed space activities and many other activities on the basis of having power together with other nations over the forces of nature for the benefit of mankind, and we have not used them for major coercive positions of benefit to ourselves."

A clear statement of policy on this adhered to by nations, used as a guide to judge the actions of nations, it seems to me can give much assurance to the less developed nations as they join in these international efforts.

The second concept, it seems to me, is to make clear in policy statements and in actions taken under the statements, the value that the competent scientist and researcher has in areas beyond the extension of scientific or theoretical knowledge—that is his value to his own national leaders in their international relationships, his value to engineers and others who are working in the developmental areas to determine that boundary beyond which scientific knowledge at that time and place does not permit the reliable operation of machines, equipment and systems.

It seems to me that this concept has a very great advantage in encouraging the kind of intellectual leadership and exchanges in peripheral areas around this scientific group. It is the intellectual exchanges between disciplines, it seems to me, that we need to build on in a cooperative way.

The third concept, it seems to me, is some policy that makes clear and can be relied on that advances in the education of future generations is directly linked to the existence and participation of scientists in a country, particularly at the graduate level. This is not clearly accepted around the world and if nations over a period of years could work under this concept, and if international policy could make it easier for national leaders to apply resources in this way, under this policy, I believe a substantial advance could be made on which further advance could be made later.

It seems to me further that we need more in the way of a policy statement and commitment along the line of investments in basic

research support, support of basic research is an investment that will yield important economic returns. Perhaps an international policy that makes this clear and permits national leaders to follow within their own nations this international policy over a period of years will yield illustrations that will permit, no matter what the economic system of the country is, a larger allocation of resources from the economic system to basic research fostering these other relationships which I have mentioned.

Finally, it seems to me that we need to find a way, and I am less clear in my mind about this, to make it clear through policy, or experience under policy, that when a nation moves from the area of scientific research, the advance of knowledge to engineering design and then on to organized use of resources, there is a competence in the area of organization and administration that needs to be brought into play for success and that nations can cooperate in these related fields, that is, in organization and administration, closely allied with engineering and science, as well as in scientific pursuit. There may be less difficulty in bringing this area of cooperation into play than in some of the more sophisticated scientific areas.

I would like to add to that something from the experience we had at the National Aeronautics and Space Administration. We did try very hard to spread the most difficult problems we had over a very large number of able minds, supporting a wide variety of scientific and semiscientific work on the campuses of many universities and then endeavoring, through the sustaining university program, to foster an idea there was a legitimate and proper role for research scientists to play in connection with the application of their knowledge—it was to allocate some small part of their time to use their keen intellects to try to find ways to help an engineer who was working at the very forefront of scientific knowledge on materials or environment to know that point beyond which he should not proceed, beyond which he would get unreliability and toward which he must endeavor to approach, otherwise he would drop short of what was possible.

It was very hard in the early years to get this accepted by scientists. Many of them said, "Our work is too important to divert our time and attention to application." But there was a growing awareness that this was a legitimate and proper role, and it seems to me that it is a clarification of that interface between the brilliant and able engineer who is trying to reach out as far as he possibly can at the existing state of knowledge and who needs to know not only where the researcher thinks the front is at the moment, but the trends, where the front is going to be, say, in a few months or a few years.

That is a perfectly proper and legitimate function and one which many can accept on an intellectual basis and on a multidisciplinary basis.

INTERNATIONAL COOPERATION IN THE SOCIAL AND LIFE SCIENCES

THOMAS R. ODHIAMBO[1]

One of the most urgent questions facing us today is whether or not we can close the gap between technologically advanced nations and the developing countries while, at the same time, keeping the prosperity of the former group afloat. The poverty of some of the developing countries is so dramatically obvious that the world's conscience has been stirred to do something about it—at least to talk about it.

But the significant question is: Can one raise the real income of these countries without pulling down that of the others? Can everybody share in a new millennium of prosperity, rather than sharing poverty all around?

This sort of question was not asked before 1950—or at least the clamor for an answer had not become insistent. Indeed, before 1950 gaposis—by which I mean the phenomenon of some countries becoming richer and richer while the newer nations are becoming relatively poorer and poorer—although it was a symptom of our times, it had not become recognized as a feature of our economic landscape.

In colonial times no one talked of "gaps" for the simple reason that the prevailing economic system had no gaps. Colonial countries produced certain goods and rendered certain services; the metropolitan powers manufactured certain capital goods and rendered certain expert services; everybody shared in producing and consuming certain consumer articles. The economies of the two sets of countries were complementary and supplementary.

The situation has now radically changed, with the coming of independence. A fierce struggle to have economic independence follow on the heels of political independence has intensified. Thus, the gaposis syndrome has set in; the fear is that it may well become endemic.

A solution to this problem does not lie in simply getting rid of the gap. That is a negative approach to this seemingly intractable problem. A better approach lies in building something positively beneficial in the gap—to provide a concrete bridge between the two groups of nations.

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Technology and science have a crucial role to play in this process. The play we are talking about could take two forms: First, by applying existing knowledge to a particular science-oriented problem. Second, by identifying problems of peculiar relevance to the developing country and then developing new knowledge to solve those problems.

The first method is by far the easier path to take. Cooperation here takes a simple know-how, and it can also recruit foreign experts to put into operation the accepted solution to the particular problem in hand.

The second method is much more difficult to put into practice. There is no capsule of knowledge that one simply feeds into the factory's conveyor belt; neither is there the so-called expert. The best hope lies in using the scientific method in coming to some kind of solution.

It is in this second set of problems that one finds the most difficulty in securing international cooperation. For nations help others really to help themselves. Certainly this seems to be the truth in the democracies; aid agencies must look over their shoulders to see what the voters' reaction is to their actions abroad.

To take one particular example: trypanosomiasis, the dreaded disease of sleeping sickness, is perhaps one of the most wasteful and killer diseases of cattle in tropical Africa. It is found nowhere else in the world, for its transmitters are tsetse flies, which are not found anywhere in the world except tropical Africa. The control of the tsetse fly population will mean the long-term control of trypanosomiasis. The benefit to tropical Africa would correspondingly be enormous. Any foreign nation that would help in this task would be helping Africa in a vital sector of its economy.

Yet, what benefit would the assisting countries reap from such cooperation? There is, of course, the possibility that the receiving countries would express their gratitude for the assistance given. If this happens—and it does not always follow that such gratitude is automatically expressed by politicians, for reasons I am not competent to state—then the voters might feel appreciation for what they have done and leave it at that.

But there is another side to the story that I think we should explore. And that is the benefit the whole world of science obtains from any advance anywhere in any field of research. The fallout from techniques of controlling tsetse flies might well trigger off new thinking of how to control other insect pests by novel biological methods. It might tell us something relevant about the ecology of animal populations. This, then, is my hope in considering questions of international cooperation in science.

A new enthusiasm is spreading among the political leaders of Africa—the conviction that science and technology can be a major factor in advancing their development goals. This is not something

that has suddenly erupted; it is a conviction that has gradually taken root during the last decade as a result of several meetings held in many countries between scientists and political leaders from developing countries.

One now hears of a number of Government-sponsored experiments being implemented to organize and establish national research councils (for example in Zambia), of the formation of national academies of science (for instance, in Ghana), and of the appointment of science advisors (particularly encouraged by UNESCO).

These efforts have revealed several problems of major proportions. Firstly, there is an acute dearth of professionally qualified scientists and technologists who can apply existing corpus of knowledge to problems relevant to their countries as well as being competent to create novel frontiers of knowledge.

Second, the tools for the work in hand—the physical facilities, the equipment, and the workshop for their maintenance, alteration or updating—are woefully inadequate.

Third, the sense of isolation is overwhelming for the few scientists that are working in Africa and other developing countries, and the consequent information gap has become a physical reality.

Lastly, the environment is not conducive to scientific endeavor: there is the almost inaudible feeling that "Africa should do things and not think things." The need to solve these problems has consequently brought up the question of international cooperation as a principal technique in meeting the current demands for modernization.

Mechanisms for international science cooperation

I want to discuss the question of international cooperation, especially in the life sciences, in a wider context. Science, like love, knows no national boundaries. Indeed, international cooperation in science has become so commonplace over the centuries that it is regarded almost as a truism. I will therefore not spend any time in singing the praises of international scientific cooperation. What I will be much more concerned with is the exploration of the new mechanisms for achieving this most desirable objective.

Perhaps the oldest and most well-tried mechanisms for international cooperation in the scientific field are the following:

1. The printed book, journal or magazine—which records the research results, the conclusions, and the hypotheses of the authors on some selected scientific problem. In this way, experiences are shared and the information gap is narrowed.

2. The scientific seminar, meeting or conference—which has tended to serve more or less the same objective as the printed book, but has also a different dimension. It has the attraction of providing a platform for scientific rumor mongering in the best sense of the word,

giving participants a chance to learn of the latest and sometimes unsolidified findings from a far-off laboratory. Its greatest value, however, is in facilitating the establishment of close personal contact between scientists of the same general interests.

3. The wandering scholar—and his ability to acquire and transmit information directly. The American physicist, Robert Oppenheimer, was perhaps the most ardent exponent of the wandering scholar in recent times. He believed that the most efficient way to collaborate internationally in science was to let young scientists work in good institutions, let them wrap up the best information inside them, and then encourage them to work in other good institutions elsewhere. Post-doctoral fellowships and allied schemes are intended to do something of this sort.

4. Patenting and the selling of knowhow.

These are still potent weapons. Nowhere are they most needed than in the developing countries. And nowhere have I recently seen a more enthusiastic support of these mechanisms for a developing region than in the report by Dr. Glenn T. Seaborg, Chairman of the U.S. Atomic Energy Commission, in the course of discussing his 1970 "scientific safari to Africa."

There are other weapons, newer mechanisms that are being tried for their efficacy—not becuase the older methods no longer work but for the reason that the pace of scientific advance has become hotter and those that had not started on the historical touch-line with the scientific tradition are likely to be left behind ever more hopelessly.

Centers of excellence

The newer mechanisms that appear most exciting, at least in their international implications, are those that have been grouped under the omnibus term of "centers of excellence" or "concentrated research centers" or some similar terminology. These centers are certainly important for the physical sciences as well as the life sciences. But for the latter they hold a special significance.

While the physical sciences are basic sciences in the sense that the order the investigator studies is of a high degree, is more or less predictable, and is subject to mathematical laws, the life sciences have an additional dimension—what we shall losely call the "life principle"—that disturbs or alters this physico-mathematical order and requires the biologist to be more than a physical scientist. In this situation the possibility of constant interaction between the physical scientist and the biologist, one variety of a biologist (for example, an ecologist) and another variety (for instance, a biochemist) is vitally important. Consequently, it is worth examining the centers of excellence with this possibility in mind.

"Centers of excellence" is a generic term containing under its umbrella at least three main types of concentrated research centers:

1. The model provided by the International Centre for Theoretical Physics, sited at Trieste in Italy, which I shall call the Trieste model of research centers.

2. The type exemplified by the high-powered, mission-oriented research institutes sponsored by the Ford and Rockefeller Foundations; for example, the International Rice Research Institute (at Los Banos in the Philippines), the International Maize and Wheat Improvement Centre (at Chapingo in Mexico), and the newly established International Institute of Tropical Agriculture (at Ibadan in Nigeria). I will call these the foundation-type research centers.

3. The new model exemplified by the International Centre of Insect Physiology and Ecology (ICIPE) located in Nairobi in Kenya—the ICIPE model.

I will take these models in turn and discuss their broad objectives and their implications for maximizing international scientific cooperation.

The Trieste model of research centres

I discuss the Trieste center with utmost humility as its first director—Prof. Abdus Salam is with us here.

One of the most intractable problems of the scientific community in developing countries is the feeling of intellectual isolation and its twin, isolationism. Professionally qualified scientists in any one field are few and far between; they rarely visit each other's laboratories; there are few high-calibre scientific societies where one can test out his intellectual muscles before his peers; funds are extremely hard to come by for visiting advanced laboratories abroad where one can both explain one's research work and derive new sustenance in one's special field; and, as soon as one shows unusual potential as a scientist, he is pushed on to doing unproductive work—administration, committee work, et cetera. No wonder many scientists in developing countries eventually assume an isolationist trend; they fear, perhaps unconsciously, to expose to the world any inadequacy in their scientific achievement.

The solution—or one of the solutions—to this mammoth problem is to mount a fundamental attack on the impedimenta on the way to the maintenance of prolonged contact between promising young scientists in developing countries and the best research work going on in the whole world, including that in the developing countries themselves. This is the task that Professor Salam of Pakistan set himself to achieve.

In 1962, Professor Salam established the International Centre for Theoretical Physics at Trieste. At the same time he created a totally

new fellowship scheme of associateships. The principal concept behind the scheme is that the Trieste centre is a permanent meeting place of young scientists of proved potential and ability, who came from time to time over a period of several years to update their knowledge and find new inspiration for their own research programs back at home.

Active scientists from developing countries are granted associateships to enable them to work at Trieste for short or long periods of up to several months, extended over several years, to attend in-depth courses of advanced study in some topic of special interest, to participate in a program of seminars by distinguished visiting scholasr, and to do some quiet thinking on their own current research The cost for such attendance are met by the centre; the only obligation on the part of the grantee is that he continues his normal duties as a university teacher or research worker during his periodical return to his own country.

The strength and significance of this model lies in its providing a sort of traveling fellowship in packages spread over a relatively long period, thus ensuring that the young scientist stabilizes his own programs at home while feeling that he is very much part of the intellectual world. Indeed, the cross-linkages that have developed among the "graduates" of the Trieste Centre have created the conditions for spreading the intellectual peerage in theoretical physics over a wider sector of the globe than one would have imagined possible even a few years back.

The associates have now become an international club of accomplished scientists, whose members are familiar to each other, and who have worked out for themselves an international language of understanding that is likely to prove of potent value in the next phase of the advance of physics.

This model is capable of breaking the back of the problem that Professor Salam set out to solve. It could be repeated in other physico-mathematical sciences. I am not certain that it could make such an impact in the biological sciences. The updating of knowledge in the latter, and certainly the communication of worthwhile information, requires that this be done through some form of symbiosis: by imbibing new information, by learning new techniques and procedures through some process of apprenticeship, by actually going and working in a laboratory with your colleagues and scientific peers. The life process is so subject to infinite variations by environmental factors that the search for universal biological laws is not the exciting prospect that it is in the physical sciences.

The foundation-type research center

The International Institute of Tropical Agriculture (IITA) was established in July 1968 as an autonomous, non-profit-making, tax-

exempt Nigerian corporation. It forms part of an international network of agricultural reaserch institutes sponsored and largely financed by the Rockefeller and Ford Foundations.

These are the International Rice Research Institute (IRRI) in the Philippines, which over the last 8 years has pioneered the development of new rice varieties and has produced the technology that has led to the "green revolution" now sweeping through Southeast Asia; the International Maize and Wheat Improvement Center (CIMMYT) in Mexico, which has spread out to many areas of the world new varieties of wheat and maize that have a much higher yield than traditional varieties; the International Center for Tropical Agriculture (CIAT), now under construction in Cali in Colombia, which will focus its attention on the improvement of tropical pasture and of tropical livestock production; and the IITA, whose major concentration will be the research and socio-economic studies necessary to ensure the attainment of improved and sustained yields of food crops under tropical humid conditions. This network offers one of the most striking physical realities of the visionary foresight of some of the philanthropic foundations.

The traditional pattern of farming the soils of the humid tropics consisting of a system of shifting cultivation, in which the farmer clears an area of its bush and undergrowth and plants it with his crop, from which he gets his living for 2 or 3 years by which time the soil is likely to become too unproductive for further cultivation. The farmer then opens up a new area, while leaving the old land on fallow for 10 to 15 years, when it is once again ready for a repeat performance.

This cultural practice has served tropical peoples well; but with the rising population and widening horizons, the pressure on land has become increasingly great. Consequently, even in mid-Africa where population problems are still minimal, the period of fertility-recovering fallow has steadily contracted, and there has been a consequent drastic decrease in crop yields.

To aggravate this situation even further, the high temperature and very humid conditions that persist throughout the year in the humid tropics accelerate the chemical and microbial processes in the soil, and the organic matter is decomposed much more rapidly than in more temperate regions. Leaching of the nutrients is on a large scale. The torrential rains run off with the best top strata of the soil, leaving lateritic soils that are much more useful for making bricks and plastering walls than for sustaining plant production.

Finally, the tropical insects, the luscious weeds, and the plant diseases all combine to make final crop yields only a fraction of what they could otherwise be. In this situation, the use of fertilizers may not only be wasteful—providing as it is more plant material for the insects

and the diseases to thrive on—but could effectively block the peasant farmer's mind from accepting innovations that do not substantially add to his income.

The IITA has been established with the aim of reversing these trends. Its broad objective is to increase the yields and improve the nutritional content of the basic food crops in the humid tropics. It is also aimed at developing new soil and crop management practices that will stabilize the peculiar soil/conditions of the tropics and thus lead to a sustained and permanent cycle of cultivation.

Substantial research has been done on the cash crops one is familiar with in the tropics, such as cacao and oil palm, and these are now a valuable currency in the foreign exchange market for tropical countries in Africa. Relatively little research has been accomplished on the basic food crops of the humid tropics: the protein-rich grain legumes (such as cowpeas and soya beans); the vegetable crops (for instance, eggplant, green beans and tomatoes); and the root crops (cassava, yams and sweet potatoes). Much more research needs to be done in these areas.

There are intensive programs of research on the cereals, such as those at IRRI and CIMMYT and the cereal improvement programs initiated by a variety of agencies (U.S. Agency for International Development, the Rockefeller Foundation, and government ministries) at Serere (in Uganda) and Ahmadu Bello University (in Nigeria). But no such programs exist for the other major food crops.

The IITA will focus its scientific manpower on the exploration of basic germ plasm, improved genetic combinations, advanced breeding materials, and the most productive and economical breeding methods for these crops. It aims to determine which individual crops are potentially most important in terms of productivity, utility in cropping systems, economic returns and contributions to improved human nutrition. This is a giant task. It has made the sponsors of the institute look to its tools for the task in hand.

The sponsors have early recognized the dearth of qualified personnel in the field of agricultural sciences in the tropics. They set its major goal at doing high quality research work with the purposeful aim of solving a pressing and long term basic agricultural problem in the humid tropics. But at the same time, the sponsors have taken measures that might well alleviate the shortage of scientific brainpower in Africa by helping to train graduate students into agricultural research scientists, who may then return to the agricultural research organizations in their own countries. They help also by sponsoring popular symposia, workshops, and seminars on various aspects of food agriculture; and by providing a documentation and information center on soil management and food crop production in the tropics.

The real signficance of the IITA model for advanced research is its mission orientation in a relatively narrow field of endeavor. Once having identified the problem area, the sponsors have erected the necessary research plant, endowed it with first-class brains, and lavished on it the equipment and funds necessary to do so. They have done the identification of the problem, the gathering together of the brains, and they have provided most of the funds.

The IITA is sited on a 2,200 acre locality, on which is being erected a water reservoir with the capacity of 350 million gallons for an experimental irrigation program and research in soil-water relationships. Its staff, when fully developed, will number 400, of which over 30 will be resident senior investigators covering nearly as many specialties and fields, 100 research assistants, and 100 technicians.

The capital cost will total something like $17 million; and its annual recurrent costs will amount to about $4 million a year. In terms of funds annually devoted to research in industralized countries, these amounts are a mere drop in the ocean. In terms of the developing countries, and in its implications of orienting research talent on problems of relevance to developing questions, this type of scientific institute is of paramount importance far more than the cold figures of total investment convey. It acts as a beacon, guiding the search of the newer nations for an efficient scientific tool to use in severing the bonds that tie them to poverty.

The ICIPE model

In a letter dated October 7, 1966, the editor of the weekly magazine "Science," Dr. P. J. Abelson, invited me to contribute a review article on "Science for the Newer Developing Countries" for his magazine. He explained that Science had, over the years, published articles on other developing areas—in Latin America and Asia—and he thought it might be instructive to have a similar exercise for Africa. The review was to be submitted by February 1, 1967.

I took up the challenge as it offered an excellent opportunity to voice the anxieties and frustrations the young scientific community in mid-Africa was feeling at about that time—the time of the massive attainment of independence, and the time of the equal realization of the massive morass of poverty afflicting these proud new nations.

I will not attempt to review what I said then. I will, however, restate and update the problem as it relates to the problems of international cooperation.

A most vital circumstance that one should keep in mind is that some extremely good research was done in colonial times in Africa—in the Congo (Kinshasa), in Senegal, in Nigeria, in Ghana, and in the three east African countries (Tanzania, Uganda and Kenya). Indeed, in east Africa, the colonial power left a most impressive infrastructure of agricultural and medical research institutes.

However, the foundations on which these were built were extremely weak. I refer to trained indigenous scientists. Hardly any qualified scientists with a productive and creative capacity were left on the laboratory benches at the time of political independence, and few were on the horizon.

The university institutions, the cradle of such scholars, was hardly geared to a tempo and condition necessary to nurture the graduate schools that would produce such talented scientists. Consequently, in spite of some outstanding research having been accomplished in the African continent by the Colonial power, little trained scientific manpower was left behind to carry on the good work.

Perhaps even more important is the question of relevance—apart from the question of excellence. How relevant was the research being done related to the needs of the country? Had anyone surveyed the field and worked out the consequent priorities in research? Who decided the priorities, who were the peers of the scientists working in Africa, and so on?

The biological problems in Africa are vast and likely to inspire on with a sense of frustration—frustration whether in fact the available scientific manpower in Africa will ever make a substantial dent in solving them. In the medical and natural resources fields there are countless large questions awaiting both old and new tools and new hypotheses to tackle them—nutritional problems, parasitological and infectious diseases, psychosomatic disturbances, tropical animal diseases, tropical ecology and its bearing on questions of the conservation of natural resources, sustained productivity of tropical waters and tropical soils, utilization of forest products, the overwhelming influence of insects on tropical life and history. One could go on and on listing these major biological stumbling blocks on the path of African development.

Obviously, international cooperation is a necessary factor in the strategy for tackling these problems. Africa left alone with its meager scientific resources (human, physical and financial) will take ages to solve these problems while the rest of the world whizzes past in the next decade of development.

It is against this back-cloth that the ICIPE was born.

We started with the basic assumption that Africa needs to concentrate its scarce scientific resources in a few very large centers, each with a sharply focussed research goal, and each employing a multidisciplinary approach in tackling particular problems over a period of time. In this way, it was possible to see tangible advances being made.

A second basic assumption was that informed Africans themselves must decide the research areas of priority—uninfluenced, in the first instance, by the consideration of foreign centers of advanced sig-

nificance. For the ICIPE, I believe, is the first center of excellence sited in a developing country which has had its basic philosophy for a scientific program initially decided on by scientists in the developing country itself.

The hope, therefore, is that such an institute is regarded very much as a part of the developing world and is most likely to have a profound influence on development in this part of the world. Having said that, I would like to emphasize that the sponsors of the ICIPE have so designed the ICIPE that in its operations and support it is truly an international center of advanced research.

The ICIPE was incorporated in April 1970 as a nonprofitmaking company under the laws of Kenya in such a way that it is "a center of advanced research meeting the highest standards of world science; that the center should have close cooperation with the scientific community in Africa and thus contribute to the development of the latter; that the center should have as one of its central aims the making of discoveries and inventions that might lead to the design of new methods for insect pest control; and that the research projects carried out at the ICIPE should capitalize on the wealth of research opportunities that exist in the insect and plant life of eastern Africa."

Insects (and their relatives, such as ticks) are vitally important in the economy and health of tropical Africa—and, indeed, in other tropical regions. The devastations resultant from insect attack on crops and livestock is enormous; the debilitating and hazardous influence of diseases transmitted by insects to man are still serious in Africa.

The long term solution of these problems needs a deep knowledge of the biology of the insects concerned. Few research establishments in Africa, by the very nature of their constitution and budgeting, can concentrate their facilities and resources on long range research. The sponsors of the ICIPE have decided to concentrate all their resources and facilities in long range research in those aspects of insects ecology and physiology that contain the best promise for pest control without profound side ecological effects.

The central aim of the ICIPE is consequently to carry out fundamental research of high quality with a definite practical goal sharply in focus. Unless such a center has certain applied missions which are relevent to the development of Africa, it would have been in danger of becoming yet another academic center in which individual investigators pursue their own interests, publish erudite articles in scientific journals, but whose efforts do not effectively close the gap between our lack of knowledge of tropical biological problems in this area and the urgent development problems of that region. The sponsors of the ICIPE did not favor the idea of free-wheeling research as the core of the center's activities.

A number of innovations in its organization characterize the ICIPE as a unique scientific institute and as one of the finest examples of international cooperation:

1. Its main research workers consist of a pool of young talented post-doctoral researchers in many fields (insect ecology, genetics, organic chemistry, biochemistry, biophysics, phys endocrinology and pathology) from many lands. The criterion f ce is quality of graduate work, potentiality, and talent as a scientist. This yardstick is used by an international selection panel on candidates from Africa, Europe, North America, Asia and elsewhere. When the centre is fully functional it will have, at any one time, 30 or so such workers at the ICIPE Research Centre in Nairobi. These research fellows will be given appointments of 2 to 3 years, and they will then be encouraged to go back to their own home countries to continue their careers. In this way, an international pool of highly informed and motivated scientists of high caliber will be built up in Africa and around the world. Such a scheme of post-doctoral research in developing countries has been graphically described by Dr. Carl Dierassi, professor of chemistry at Stanford University, who is one of the pioneering spirits behind the ICIPE.

2. The research workers perform their missions with the guidance of outstanding world scientists who have accepted appointments as directors of research. These distinguished scientists have agreed to come to Nairobi two or three times a year to initiate, conduct, and advise on research priorities and in reviewing the scientific activities of the centre. At the same time they have agreed to accept in their own home laboratories talented young African scientists who need further training before joining the ICIPE as research workers.

3. The individual support of the ICIPE comes from over 50 scientists, policy makers, and other distinguished men of affairs in many countries (Kenya, Uganda, Tanzania, Zambia, Japan, Australia, Israel, Switzerland, France, Germany, U.S.S.R., Czechoslovakia, the Netherlands, Sweden, United Kingdom, Canada and the United States of America). They help to give the ICIPE a legal entity.

4. The ICIPE is advised and supported by an international committee, whose members are nominated by national academies of science and other scientific organizations in many countries. This is a novel device. It is specifically charged, among other things with guaranteeing the high quality of the work performed at the ICIPE Research Centre in Nairobi. The American Academy of Arts and Sciences and the U.S. National Academy of Sciences, and in addition the East African Academy, the Max Planck Society of the Federal Republic of Germany, the Royal Netherlands Academy of Sciences and Letters, the Israel Academy of Sciences and Humanities, the Royal Swedish Academy of Sciences, the Royal Swedish Academy of Engineering

Sciences, the Australian Academy of Science, the Czechoslovak Academy of Sciences, the Japan Science Council, the Centre Nationale de la Recherche Scientifique, and the Office de la Recherche Scientifique et Technique Outre-Mer—all these were among the first to give the ICIPE tangible and sustained support. Other academies and scientific organizations have shown similar interest and will soon formally join in this international enterprise.

5. The ICIPE is under the supervision of a governing board of international composition. Its nine members come from seven countries, with Professor Carroll L. Wilson, Director of the Alfred P. Sloan School of Management at the Massachusetts Institute of Technology, as its Chairman. Professor Wilson is present today. The vice chairman is Dr. Wilbert K. Chagula, himself a trained scientist, but until August 1970, Principal of the University of Dar-es-Salaam in Tanzania, and now Minister of Water Development and Power in the Tanzanian Cabinet.

Nairobi was chosen as the location for the ICIPE for several reasons. It is an ideal natural laboratory for insect study having an equatorial photoperiod, permitting almost year-round investigations of insects; it has a wide range of climate from tropical monsoon to alpine, and a rich diversity of insects within 200 kilometers of the city. In terms of service and access, Nairobi, with its university and agricultural research organizations, modern city housing and schools, beautiful countryside and climate and an excellent worldwide communication system is a most agreeable place in which to live and work.

I should like to say that the ICIPE is not strictly an applied research institute. Yet, its broad objectives relate to the discovery of knowledge which may fill the critical gaps which now exist in our understanding of the characteristics and biology of the target insect species of great economic importance as pests—tsetse flies ticks, termites, mosquitoes, to name only a few. Until now the world has relied heavily for pest control on broad-spectrum chemical poisons which killed all insect species—whether foe (pests) or friend (insects that eat or parasitize other insects)—and did untold damage to the quality of our environment.

It did not require deep understanding of insect reproductive physiology, genetics, sensory physiology, population dynamics and the like to determine whether a chemical pesticide was lethal or not. The question simply was whether the insect keeled over or not when exposed to the chemical. But we now see rapidly accumulating evidence of the severe environmental side effects of persistent chlorinated hydrocarbons. We also observe the increasing resistance of many pest species resulting from mutations in which resistant individuals survived and multiplied.

New avenues for investigations are now called for. Biological control through the release of sterile males is one; but it is only effective in a very few specialized cases. Other avenues are through the use of insect predators, parasites and diseases; but again the potential may be limited and the risks of unintended environmental side effects are large. Yet another avenue is through the selection of species of crops and livestock resistant to insect attack; but this is slow and often the insect pest overcomes the resistance barrier.

The avenue that the ICIPE is pioneering is to seek species-specific controls which do not affect other insects, that do not leave persistent residues, and that have minimal ecological pertubations.

We must fill the gaps in our knowledge in the loop provided by insect ecology, insect physiology, integrated control, environmental quality. We must now understand a great deal about the complex development process through the egg, the larva, the pupa, and the adult insect, a process that is amenable to disturbance or interruption by the introduction of insect hormones at the wrong time, for example.

We must have a deeper knowledge of reproductive physiology and genetics, as it offers the prospect of interfering with the mating process or altering the sex ratio among males and females in the offspring. We must possess a clearer understanding of the remarkable sensory organs and the rich system of chemical communication among insect communities of the same species, as it may give us a chance to use false chemical signals to lure insects to their destruction.

These and other promising leads of the same nature will be assiduously followed by the band of talented research investigators, trained in many disciplines, at the ICIPE. The general direction of their work will be beaconed by the deep insights and judgments of the senior scientists who are the directors of research at the ICIPE Research Centre.

In addition to the advance of knowledge in these fields, a primary aim of the ICIPE is to bring into a developing region—in this case, Africa—a center of advanced research where African postdoctoral scientists can find professionally rewarding activity on problems of (a) great importance to the economic development of Africa (and many other parts of the world) but also (b) great intellectual interest. Such a center of excellence could have a powerful effect on the recognition of science by African leaders as a practical tool. It may also encourage African students to study science, and will undoubtedly attract talented African scientists to pursue their profession in their own continent and thus dam the brain drain before it bursts upon us in earnest.

The vision of the sponsors of the ICIPE is a vast one. Have they the resources to bring it into the realm of reality? The human scientific resources are assured. The governing board has assembled together

the most distinguished group of insect scientists in the world to act as directors of research; the postdoctoral investigators are being appointed on the basis of their outstanding graduate performance and their proved potential as independent research workers; training schemes for graduate technicians is being launched; and the ICIPE Research Centre will rely on trained business managers and other administrative cadres for its day-to-day administration.

The research center will be adequately provided with all the equipment that it needs to use for the whole range of its activities. Excellence and relevance are the criteria of the ICIPE in selecting and directing its resources to achieve its goal. Thus, the ICIPE is to possess a first-class research plant, the best brains in the field of its endeavor, and the most efficient administrative machinery for the job in hand.

The question of funding is, however, giving the ICIPE governing board a giant-sized headache. Ideally, the ICIPE needs one or two very large financial backers—in the same way that the foundation-type research centers are being financed. The genesis of the ICIPE, however, and the totally unusual design of the ICIPE has made this problematical.

In this situation, the ICIPE governing board has turned to international sources of finance and the government aid agencies as the major sources of finance, with the various academies being responsible for raising funds for some fellowships. The most difficult items on the financial agenda of the ICIPE are capital funds ($1 million), equipment ($2 million), and the operational costs (annually $1 million) of the Centre. If these can be provided on a long-term basis, then the sponsors will have been given a rare opportunity to demonstrate the utility of this unique experiment in international cooperation and institutional development.

I started this section of my discussion by advancing the possibility that centers of excellence might be of special value as providing the right kind of environment where there is constant interaction between the physical scientist and the biologist. In this way, new streams of thought might be provoked that would otherwise lie unexcited. The ICIPE Research Centre will provide just this kind of cultural environment—where physicists, biophysicists, and organic chemists will rub shoulders with ecologists, physiologists, and endocrinologists; and where the few carefully selected problems will be investigated from all sides by a whole range of tools.

For example, the ICIPE will soon be mounting a long-term research project on insect migration—one biological phenomenon of insect life of great importance in the pest status of certain species. In this case, our studies will be focused on armyworm, a group of moths whose caterpillars devastate a considerable proportion of tropical and subtropical pastureland and agricultural crops every year when these

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ıngsters amass themselves into veritable hordes of feeding machines, wing down whatever is in their path.

A team of ICIPE research workers will be studying the field biology the adult moths themselves, investigating their flight range, the vironmental factors that regulate their seemingly unidirectional mass vement, and the periodical phenomenon of their apparent evidence, ng light-trapping techniques, aerial plankton collecting methods, bile radar identification procedures, and high-resolution Doppler hniques. Other workers will be conducting laboratory and wind nnel experiments on the biophysics and biochemistry of their flights; ile others will be concentrating on elucidating the hormonal and ner physiological factors that regulate the migration instinct of the ths; and yet others will be studying the possibility that chemical ssengers might be important in the congress of the moths.

Thus, a whole range of biological and physical tools will be brought bear on understanding the migration biology of the adult moth.

It remains to the assess whether the ICIOE model can be applied in ner areas of biological research. One cannot pronounce categorically thisat the moment, since it will take at least another 5 years for full story and potentiality of the ICIPE to enfold. But the iceberg e can see is a marvelous sight; and we are beckonig to the world join us in the search for what is still to emerge in this crucial historical oment for the oriented and applied development of the biological ences in the newer nations.

I wish now to turn my attention to a question I begged at the ginning of this discussion. Just how valuable is international scitific cooperation to any particular nation? The question can be anvered in many ways by, for example, stating that science is fostered st in an environment of free discussion and consultation, that scitists build on the foundation of discoveries laid by their academic edecessors, that science is an international language that can only velop through all the participating speakers learning the internanal rules of the language and so on and so forth. A number of uths are imbedded in these assertions, and they have undoubtedly ayed a role in fashioning science into the pregnant role it is playing the life of the world today.

But international scientific cooperation has another role to play ow. International cooperation in the life sciences can help the new ations to leap-frog into the industrial age quickly, if the new nations diciously utilize the existing knowledge and techniques in solving neir particular problems. The older nations also stand to gain by ackling new questions in new situations using the tools with which ney are already familiar. But equally critical to the rest of the world the probability that such cooperation will bring (from the developng countries) into the international scientific community a new talnt, a new genius, a new mode of looking at problems until now shut

off from participation and from communing. If this can be facilitated, the world brainpower will then be the richer.

The central goal of the social sciences

A new voice is beginning to be heard emerging in Africa which rejects both the traditional Africa and the technologically oriented Africa. This is exemplified by a number of scholars. The writings of the Ugandan poet, Okot p'Bitek exemplify these in a biting satirical manner. Lawino, the illiterate wife of the book-learned Ocol, sings a lament to Ocol with his new fangled civilized ways:

The white man's stoves
Are good for cooking
White men's food;
For cooking tasteless
Bloodless meat of cows
That were killed many years
And left in the ice
To rot!

For frying an egg
Which when ready
Is slimy like mucus.
For boiling hairy chicken
In saltless water.
You think you are chewing paper!
And the bones of the leg
Contain only clotted blood
And when you bite
The tip of the bones of the leg
It makes no crackling sound,
It tastes like earth!

The white man's stoves
Are for boiling cabbages
And for baking the light spongy thing
They call bread.
They are for warming up
Tinned beef, tinned fish,
Tinned frogs, tinned snakes,
Tinned peas, tinned beans,
Big broad beans
Tasteless like the cooro!
They are for preparing
Foods for the toothless,
For infants and invalids.
It is for making tea or coffee!

Later, Ocol replies to Lawino's lament by laughing at the traditional African life. He asks, "What is Africa?" And he gives his own formula for that question.

Timid,
Unadventurous,
Scared of the unbeaten track,
Unweaned,
Clinging to mother's milkless breasts,
Clinging to brother,
To uncle, to clan,
To tribe,
To blackness,
To Africa.

Africa,
This rich granary,
Of taboos, customs,
Traditions * * *.

These are bitter words, and yet in contrast, many African leaders are more and more looking toward science as a tool for development, but at the same are convinced that social experience must be brought to play to temper the brutalities of technology. This does not seem to be contradictory. Technology is the local interpretation and use of science. The social implications of these technologies must be studied if we are to avoid the worst side effects of such advances.

One would therefore be tempted to suggest that perhaps one of the central objectives of research in the social sciences should be to answer the following cardinal questions:

(1) Can the social framework absorb the advances in the life sciences (and other sciences) that are being brought about by the discoverers and innovators?

(2) Can the social scientists make discoveries of the human mind and his way of organizing his fellow men that would give him a conscience beyond mere physical and economic development?

(3) How best can mankind be trained to feel that the environment and its natural resources have to be held in trust for posterity?

This is a task that transcends the local enumeration of statistics or the study of the many -isms that plague the world. It needs the cooperation of all, for the social sciences—if indeed these studies have attained the level at which they can be termed sciences—are still very young and have yet to contribute significantly to the well-being of mankind.

Perhaps one proposal which should be considered seriously is that ery scientific institute should have a small team of social scientists tegrated into the work of the scientific investigators—sifting, evalting, passing value judgments, and studying the social implications any new findings of the scientist.

onclusion

I have tried to portray the problems and new mechanisms of interational scientific cooperation in the life sciences from the point of iew of the developing countries, and especially Africa.

I have done this for two main reasons. Firstly, because my own nowledge of the world is not that all-encompassing and I wanted to peak where I felt most confidence.

Secondly, I am convinced that in formulating scientific policies— hether national or international—one must consider the whole world s his stage. Science is indivisible. A policy decision taken in one ountry will ultimately affect all other countries in one way or another.

To me, a most gratifying circumstance is that this "natural law" is ecognized in the United States; and in their attempt to reevaluate and efashion their national scientific policy they have set out to share heir thoughts with other scientists from other countries. This, then, s a beautiful demonstration of the spirit of international cooperation.

THE LEGISLATIVE ROLE IN SCIENCE POLICY

ALLISTER GROSART [1]

It is a great honour to take part in this prestigious conference sponsored by the internationally renowned Committee on Science and Astronautics. I was here last year as a listener and member of the audience. This year I have been promoted to the platform. The dual role in these two years recalls the comment of that great English wit, Augustine Birrell, who when asked if a speech he had made had been a success replied: "The speech was a success but the audience was a failure".

I hope that will not be the case today. I am sure it will not, because I can rely on your tolerance towards a layman who, as a very non-scientific member of the Committee on Science Policy of the Canadian Senate, has been lured out of his depth into the deep seas of science and science policy.

Whatever my reception here, it will perforce be mild compared to that which the first volume of our Senate Committee's report has received, in certain scientific circles in Canada. This is not surprising, of course, in view of the fact that the whole tenor of this first volume is highly critical of Canadian science policy over the past 50 years, and by implication of many of the individual and institutional policy-makers involved. It was not easy to decide to publish the conclusions we had reached because it was inevitable that they would give offence. We did so only because we had become convinced that it would be calamitous if the new round of science policy-making that is about to begin was based on the conventional wisdom of the past rather than on the abundant evidence in our own experience (and in that of other countries) that Canada has not and never had—an overall science policy. We cannot hope therefore to use effectively our limited financial resources in support of science and technlogy (and particularly research and development) on the basis of the existing pattern of diffuse, ad hoc, responses to funding and performance claimants on the public purse.

Not without some concern that we would be misunderstood or misrepresented we have come out for an overall science policy at the highest political levels—that is to say for a macropolicy approach to Canadian science policy to rationalize in advance the hundreds—

[1] Member of Parliament, Canada; chairman of Steering Committee on Science Policy, Canadian Senate, Ottawa, Canada.

perhaps thousands—of micropolicies which have resulted in what we called "Science Policy by Accident".

We found that in comparison with other OECD countries both our public and gross funding of R & D was inadequate; that Canada was at the bottom of the list in both funding and performance of R & D by business enterprise and therefore in innovation generally; that we are doing far too much of our R & D in government laboratories; that we have probably the lowest rate of transfer of R & D to industry from government laboratories and universities; that our new Science Council (1966) is operating "in a vacuum"; that our National Research Council, despite its accomplishments and international repute among scientists, has not fulfilled its intended role in terms of national scientific policy.

I need hardly say that there have been some howls of protests—particularly from fundamental scientists—who seem to find it so hard to understand, that political or public insistence on the application of social, economic and technological criteria to overall national scientific productivity is fully consonant with staunch public support of basic, curiosity-oriented research.

Indeed, from a political point of view it seems to me that pure scientists would have far more to gain than lose from a well planned overall science policy. This is so, because pure science is the one area in which the political decision to spend is exempt from the normal requirement of cost-benefit justification. Here, as nowhere else in the national budget can a case be made that basic research has a clear claim on public funding without strings attached. I need hardly say that all claimants for public funding in science affairs would like their funds "without strings attached". Fortunately or unfortunately, depending I suppose on one's viewpoint, this runs counter to the essential requirement of accountability of all public servants for the expenditure of public funds.

All of which leads directly to the whole question of the role and responsibility of the individual legislator in and for national science policy. I am sure that in most of the countries represented at this Conference, the essence of the control of the executive branch by elected representatives is through the control of the purse—the annual Budget, Appropriations or whatever it may be called.

This, of course, assumes that in each legislature there are individual members or groups of members who have expert knowledge of the many objects of public expenditure as they appear in the annual estimates. There will be a few, perhaps, who will have a special competence to relate the totality of suggested expenditures to its impact on the national economy and the underlying societal values. But they will be few. The majority of members will have special competence in one or two particular spending areas—agriculture, social welfare,

health, education, industry, foreign affairs, justice, resources—and so on. This is so because these have for a long time been major objects of public expenditure. By and large they have high visibility in the sense that the expenditures can be readily apprehended and assessed in relation to the general or regional interests affected. The whole concept of representative government is based on the assumption that all claims on the public purse will have effective examination by members of the legislature. This sytem, with its checks and balances, claims and counter-claims, has worked tolerably well where the areas of expenditure have grown gradually with matching development of competence on the part of legislators.

The system tends to break down, however, when a completely new and expensive program or project is introduced. Its very newness finds the legislators unprepared to assess critically. It gets easy passage through the legislature because members do not feel qualified to discuss it critically; have neither the time or inclination to do their homework and therefore have no way of knowing its general or regional implications. The history of politics is full of such cases and the inevitable matching story of false starts, colossal waste of public money and even scandals.

The studies we made in the Science Policy Committee of our Canadian record and that of other countries in national science policy seems to suggest that science and technology (and their handmaidens research and development) are often in this category. Certainly in all the industrialized countries which are engaged in what has been called "the Science and Technology race", there have been in recent years fantastic increases in the public funding of R & D activities. In our own case in Canada from $70 million in 1951 to $583 million in 1970. Here in the United States from $1,301 million in 1951 to $16,922 million in 1970.

If I read the evidence of the countries concerned correctly, I have to conclude that no executive—and certainly no legislature—was even remotely prepared to assess these new and prodigious claims on the public purse, even in such basic decisions as the respective funding responsibilities of the public and private sectors; of the most effective "mix" of performance by government agencies, universities and industry; of a viable distribution of funding between basic and applied research, development and innovation; on the proper roles of decision-makers all the way from the Cabinet through mission-oriented departments, councils, boards, Budget Bureaus and Treasury officials, to the individual scientist or institution.

This of course, is what the so-called great debate on science policy is all about. If one looks at the widely-diffuse answers to these questions that various countries have given one looks in vain for any kind of rationale that could by any stretch of the imagination be regarded

as the end product of a national science policy. Certainly not to the same extent that there is a visible fiscal, farm, welfare, or foreign affairs policy.

In Canada we had a Cabinet Science Committee which never met for ten years. We embarked on the public funding of one scientific project which was finally cancelled when there was a change of government. It took ten years for government funding of science in industry to reach the previous level. There are similar cases in other countries.

I make this case for the general impreciseness of national science policies because I believe that some, at least, of the responsibility lies with the members of national legislatures. We did not, in the initial stages of science spending, equip ourselves to make the same kinds of judgments that we were able to make in other policy fields. The reasons are understandable. The suddenness of the technological explosion, as I have already indicated in one.

Another is the complexity of the subject itself. A third is the fact that very few scientists become legislators. A fourth is that in most countries the majority of legislators are not yet aware of the fact that national science policy is now the major determinant of the whole future way of life—in both quantitative and qualitative values—of those who have entrusted to their judgment the decisions on which those values rest.

In Canada we have no Ministry of Science, and no overall science estimates. Therefore, under our system, we have no standing committee on science or science policy. Not only so, but because ours is a system of parliamentary Responsible Government in which members of the Cabinet are in the legislature, there is no Minister a member can question about overall science policy. In other countries there are ministers with specific science policy responsibility.

Two perhaps in Britain—Science and Education and the Ministry of Technology;

In France, a Minister of Industrial Development and Science;

In West Germany, a Federal Minister of Education and Science;

In Belgium, the Prime Minister is the Chairman of a Council of Ministers on Science Affairs, and is assisted by a Minister without Portfolio who is responsible for science policy and programs;

In The Netherlands, a Minister of Education and Sciences;

In Sweden, the Prime Minister is Chairman of the Science Advisory Council;

In Switzerland, however, no Minister of Science.

I confine my comparisons here to those countries the Senate Committee had the opportunity to study on the spot.

Most industrialized countries have, of course, legislative Committees which specialize in science policy. It is the exception rather than the rule for any such Committee to have before it an overall Science budget as such—an innovation which, judging from its operation in France, Belgium and the Netherlands, would seem to be a requisite for the full participation of the elected representatives in any country in the development and control of national science policy.

Here in these United States there is, as we all know, no cabinet secretary for science (although there is one for Health, Education, and Welfare). There is, I believe, no science budget in the European sense, but our American friends have, as would be expected, carried into the science policy field their genius for government by checks and balances. So we have the Bureau of the Budget concept where the science items in the departmental budgets are received as a whole, with the result that there is at least a defacto science budget available sooner or later for examination and assessment by members of Congress.

There is the Science Advisor to the President, the President's Science Advisory Committee (PSAC), the Office of Science and Technology (OST), the Federal Council for Science and Technology (FCST), the National Science Foundation (NSF) and others all making visible inputs to the national policy—illustrating magnificently the usual wonderful American anomaly of a constitutional structure which in theory shouldn't work but which in practice works rather better than some other constitutional structures which should work much better, but don't.

Our Senate Committee visited a number of countries in Europe and met many legislators interested directly in national science policy. We gathered from them that the problems of the average legislator or member of parliament are much the same everywhere:

For example the number of legislators willing to become actively involved in the making of national science policy are everywhere too few. These few make valiant efforts to enlist support from their colleagues but generally without too much success. The science-concerned members are regarded as a bit odd—specialists in the abstract rather than in the bread-and-butter issues of politics.

The subject matter of science policy is so vast, complex and pervasive that it is taken for granted that any overview of the whole is beyond the scope of the ordinary legislator. It is too easy for the Executive to persuade him that the best he can hope to do is to concern himself with it piece by piece as it comes to him as items in the budget of a department or agency.

Which leads to the suggestion that perhaps it is easier to obtain legislative approval of the science budget if it is so fragmented. This

has been put forward to justify this approach—but it leaves unanswered the larger question of legislative responsibility for the adequacy of both the quantity and quality of the science budget as a whole.

I think it is Jacques Barzun who gives the amazing example of United States expenditures on oceanography. Eight different agencies were involved in the input. The Federal Council meshed them into an overall oceanography support program. This came to Congress redivided into parts in the operating budgets of the agencies. The parts were referred eventually to 13 sub-committees of 7 House of Representatives Committees, 1 Joint Committee and 9 sub-committees of 6 Senate committees.

It could only happen, perhaps, in the United States Congress where, if my survey of comparative legislatures is valid, the U.S. Congressional Committees—substantive and appropriations—have developed a capacity for expert examination of science spending unmatched by any other legislature. I need hardly, in passing, acknowledge the debt of all who are interested in this problem to the Committee on Science and Astronautics which sponsors this meeting and to its sub-committee on Scientific Research and Development. When we add the Senate committees and the appropriations committees of both houses—one of them powerful enough to kill the ill-fated Mohole project—it would appear that American legislators, at least, are fully and effectively involved. Yet, as astute an observer as Professor Michael Regan could write as recently as 1969: "Our $17 billion for R & D is still in search of a policy. . . . No other area of discretionary expenditure is larger in today's budget, yet none is less well rationalized, less satisfactorily justified or distributed among competing claimants . . . overall national policy regarding science and technology is still not clear."

And he makes it clear that he is referring not merely to justification at the executive level but to what he calls an adequate "joint-executive-legislative frame-work".

Harvey Brooks of Harvard says one of the main reasons for a planned science budget is "to get the $17 billion under control".

It has been the privilege of our Canadian Senate Committee to hold two joint sessions with Chairman Miller's committee and we have, of course, studied the U.S. science legislative frame-work in some detail—and with considerable envy and much admiration. Compared with other national frameworks, I would have to say that if any should work, it should be it. Professor Regan's comment, therefore, seems to make it clear that we all have a long way to go before any science policy anywhere can be said to be under effective legislative control.

This does seem to me to be cause for alarm given the abundant evidence of the degree to which the interests of our constituents are bound up with national science policy.

The problems, of course, are enormous, the conflicts inevitable. The concept of control by representatives of the public is a political, not a science concept.

Yet as Derek de Solla Price says "the density of science in our culture is quadrupling during each generation". We know that science can now make man's environment, his society—man himself. We know that we must somehow achieve a synthesis between science and society—that, whether we like it or not, science policy is science politics. That being so, surely legislators as a whole dare not continue to ignore the overall policy aspects as they appear to have done so far.

Of course, the legislators are only part of whatever control mechanism may be required. Alexander King, the genial science sage of OECD, was one of the "wise men of science policy" who gave our Committee the benefit of their knowledge. He told us that the problem was a mixture of "the naivete of the natural scientist, the arrogance of the economist, the complacency of administrators and the ignorance of politicians".

And if any politician here present objects to being called "ignorant" on this account, I would suggest that he read the comments by René Dubos in a recent article entitled "The State of Our Ignorance". It was reprinted in that excellent official U.S. Government underground magazine called "Dialogue". He tells us that 70% of the particular contaminants in urban air are still unidentified and suggests that we are chasing a will o' the wisp if we think that even complete control of soot, carbon monoxide and sulphur dioxide would make very much difference to the total state of air pollution. "No one knows which air pollutants are most dangerous or where priority control should be." If these things be so, is it too much to suggest that a lot of legislators in a lot of countries are being led up a lot of garden paths.

"Can the individual and science co-exist?" asks Professor Z. Brzezinski of Columbia. His question might be paraphrased "Can the legislator and science co-exist." One would hope so.

Andrew Shonfield, Chairman of the British Social Science Research Council, says "it is out of knowledge of Society rather than Technology that the major insights about a quarter of a century away are likely to come."

And again: "Futurology, one must conclude, cannot be turned into a respectable 'hard' science merely by getting the economists and technicians to put some numbers to it. These solid-seeming, straightforward, statistical measurements acquire significance only when the speculative social imagination is applied to them".

That may be intended merely as a plea for a greater input of social science into national science policy—a point greatly emphasized in our Senate Committee Report. Yet—inherent, surely, in the concern for this synthesis between science and society, is the responsibility of the legislator—the average, ordinary, back-bench legislator who is, if he is anything, society's representative in the governmental process.

There is more than one side to this. Donald Hornig, a former Science Advisor to the President, tells us that the main reason why the American public has supported government science spending to the point where it constitutes the largest percentage of GNP of any country in the world, is that science and technology have been identified in the public mind with political, social, and economic goals.

Michael Regan drawing on research by Martha Ornstein and others, points out that the gap or gulf between Science and Society has not always been a problem. In its early days the Royal Society of Britain was a club of non-professional gentlemen interested in science, who were quite competent to discuss 4/5ths of the papers presented by other members. It was 200 years or more before its membership became limited to research scientists. In 1800 there were 100 science journals. Today there are over 100,000 in many different languages.

In the pre-technetronic world—as indeed in the legislatures of some of the newer countries today—Science as it affected Society was within the knowledge spectrum of the average legislator. How times have changed, at least in the post-industrial world!

"We so refine what we think" said the late Robert Oppenheimer, speaking as a scientist to scientists, "we so change the meaning of words . . . that scientific knowledge today is not an enrichment of the general culture . . . it is not part of the common human understanding."

It is easy, of course, to blame the scientists and technologists for doing, what to them, comes naturally—pushing back the frontiers of science wherever and however they may be pushed back. But Donald Hornig makes a telling point when he says that "putting limits on what people may or may not discharge (into the air and water) is not an R & D proplem."

We know whose it is. All the evidence of the failure of voluntary restraints on individuals and institutions throws the problem right back into the lap of the legislator.

Social moralists like Joseph Wood Krutch may tell us that the technologist is a "Sorcerer's Apprentice who does not know how to turn off what has turned on"—but the technologist can be forgiven if he replies that it is not his job to turn it off. Again we come babk to the legislator. "Let's design the 'off' switch before we turn on the 'on' switch" we were told in Canada recently by Dr. Athelstan Spilhaus,

President of the American Association for the Advancement of Science.

In our Committee's briefings in other countries we were particularly interested, of course, in learning what mechanisms legislators had developed to make themselves better informed on science policy matters.

In general, we found that these broke down into a number of procedures—not all of them, of course, found together in every country. These are:

1. *Standing Committees:* In these, legislators examine officials and sometimes special witnesses (i.e. scientists) on science items but only incidentally to the routine examination of Estimates, Budgets, bills or other matters referred by the legislature.

2. *Special Science Committees:* A good example is the Select Committee on Science and Technology of the British House of Commons constituted in the 1966–7 session and re-constituted each session since. It has unusually wide powers for a House of Commons Committee. Its terms of reference are "to consider Science and Technology and report thereon from time to time". The Congressional committees already referred to are in this category, although each is rather more specialized in its approach than the British Committee.

The Netherlands has a Standing Committee of the Second Chamber (Lower House) on Education and Sciences.

Sweden has a Supply sub-committee dealing with "Research, Education and Technology".

West Germany has a Bundestag Committee for "Science, Education Policy and Journalism". It "prepares the ground for discussion and decision" by the Bundestag. It invites experts as witnesses.

Belgium has a Joint Committee for Science Policy established in 1962 but it is said to meet infrequently.

The U.S.S.R. I find (although our committee, regrettably was not able to include it in our briefing sessions abroad) constituted in 1967 a Standing Committee of the Supreme Soviet on "Education, Science and Culture". It has the authority, held by no other Science Committee that I know of, to initiate bills as well as to study the relevant items in what, for want of a better word, I will call the state budgets.

3. *Voluntary Committees:* A third mechanism for the involvement of legislators in science affairs is the voluntary association (sometimes called a Committee) of legislators. This is a sort of science club of parliamentarians without official constitutional status. The outstanding example is, of course, the Parliamentary and Scientific Committee of the British Parliament, a non-party group which was established as long ago as 1939. It provides "permanent liaison between science bodies and Parliament as a centre for the consideration of science

matters in both Houses of Parliament." It provides members of all parties with working summaries of science subjects as they come before Parliament.

Sweden has a similar association called RIFO, its name derived from the initial letters of the words for Parliament and Research. It dates from 1959 and has some 400 members of whom 225 are legislators. A Board of ten constitutes a working Committee.

Denmark has tackled the problem head on, it would seem, by setting up a Committee of 5 legislators and 5 scientists known as the "Contact with Science Group". It arranges regular meetings between scientists and parliamentarians.

These are but random examples to indicate a growing awareness among legislators of the need for more thorough-going immersion of parliamentarians in the deep waters of science than in more traditional subjects of legislative concern.

As far as I know the *International Panel* concept is unique among legislative science committees. It does, however, point up a very important aspect of science involvement by parliamentarians which was quite often brought to the attention of our Canadian Committee when we visited other countries. Over and over again we were asked the question "How do *you* handle it?" in matters ranging all the way from total public funding of R & D to the science brain drain problems of developing countries.

It would be an understatement to say that it was a rewarding experience for the members of our Committee to be able to bounce off some of our assessments of the evidence we had heard at home on fellow legislators in other countries as well. Of course, on ministers, administrators, industrialists and academics. I would go so far as to say that we could not have written our report with any confidence without those contacts. Long ago Dr. Killian of M.I.T., made us aware that one, at least, of the international spin-offs of the S & T explosion was what he called the "eclipse of distance" between nations in both transport and communications. Today we call it the concept of the global village.

We in Canada, for example, are aware of our dilemma in international science created on the one hand by our minimal input into world science (probably not much more than 1%) and on the other by the fact that we exercise political sovereignty over the second largest national land area, and the air and atmosphere above it and; much of the Arctic shoreline, and large sectors of the Atlantic and Pacific coasts.

When we meet legislators from other countries in frank discussion—franker perhaps than takes place at any other level of international contact—we are often impressed with the similarity of the concern at our levels about input and output problems of air, sea and soil

pollution, weather modification and forecasting, nuclear fall-out and so on.

Such meetings with our colleagues take place under a surprisingly varied set of auspices. In Canada we have delegations of legislators continually coming and going to meetings of such international associations as the InterParliamentary Union, the Commonwealth Parliamentary Association, the Francophone nations, the NATO Council of Europe, the Canada-United States Parliamentary Group.

I wonder perhaps if the time is not now ripe for the organization of an international Parliamentary Science Association. From the experience of the Canadian Senate Committee the advantages would seem obvious. The Panel meeting we are attending is, I think, a farsighted recognition by Chairman Miller and his associates of the value of an international meeting of minds concerned with the very great problems poised by national science policies within the clearly evolving pattern of international science policy.

I believe that such an association would make a very real contribution to the great international science problems—the high seas and the ocean floors, disarmament, satellites, population, food and the transfer of scientific and technological resources from the affluent to the developing countries.

I put forward the suggestion today at this important international science meeting in the hope that it might be taken into consideration by some of those who are here from other countries. I know the obstacles because I have had something to do in a small way with similar organizations in other fields. I know, too, how great the rewards can be because of the immensity of the vacuum that exists in the knowledge and understanding between legislators in different countries about the procedures and mechanisms that are developing here and there in this important business of the involvement of parliamentarians in science policy making.

The available information and literature at the moment is minimal—almost non-existent. It has found practically no place in the proliferating literature on the great debate about science policy. Everywhere the stress is on the role of government—a syllogistic short cut perhaps but prone to many fallacies. In the long run it will be the generality of legislators in each individual country who will, as the elected representatives and spokesmen for society at large, resolve the conflicts between Science and Society. What a happy "technological innovation" it would be if somehow they were to find a way to join minds and hands across the boundaries of lands and waters to achieve that reconciliation for all men and all seasons.

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SCIENCE, TECHNOLOGY AND THE DEVELOPING COUNTRIES

HARRISON S. BROWN [1]

Throughout his million or so years of existence, man has been confronted by countless threats to the surivival of his tribes, villages, city-states, nations and civilizations. Hunger, pestilence and violence have continuously taken their toll and throughout history deprivation and misery have probably been the rule rather than the exception. But at no time in history has the prognosis for mankind as a whole been as alarming as it is today.

To observe simply that two-thirds of mankind lives at virtually a subsistence level—ill-fed, ill-clothed and ill-housed—is to take a superficial view of our present condition. The roots of our problems are spread deeply throughout the soil of history and when we examine the *trends* within our global society as well as its present condition, the prospects indeed appear ominous.

People are tired of reading the warnings of so-called "prophets of doom" and it is not my intention here to be such a prophet. By temperament I am more optimist than pessimist and if I really were to believe that the situation in which man now finds himself were hopeless I would not bother to write these words. Our situation is desperate, but it is not hopeless.

Before a problem can be solved the nature of the problem must be understood. Before there can be understanding, there must be recognition. One of our basic difficulties today is that the extent of understanding and recognition of our most critical global problems are woefully inadequate to permit meaningful solutions to be found.

The overriding fact which characterizes our world today is that humanity is fissioning into two major groups—the culture of affluence and the culture of poverty. The beginning of the fissioning process dates back to the time, some two centuries ago when the Industrial Revolution exploded in Western Europe. A succession of technological innovations beginning with the steam engine, increased fantastically the amount of power that man could mobilize to achieve preconceived goals. The availability of these new levels of power, coupled with a growing body of empirical observations concerning the behavior of materials, gave rise to greatly increased productivity in industry and agriculture. When systematic research made it possible

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to couple a growing scientific understanding of nature with the development of new practical technologies, productivity increased still more rapidly.

As productivity increased, material wealth increased as well. As wealth increased, health improved, death rates fell and populations increased rapidly—but seldom as rapidly as wealth. The lot of the average individual therefore, improved substantially in spite of high birth rates. Eventually, for reasons which are by no means completely understood, birth rates in these areas fell. When this happened per capita wealth increased even more rapidly than it had previously.

Those nations which were rapidly developing industrially consumed raw materials at increasing rates. Those raw materials which could not be obtained at home were imported. Further, as affluence increased, quantities of products were imported from overseas which at that time were luxuries rather than necessities. Exports were used to pay for the imports and this meant that markets had to be developed. The need for controlling sources of raw materials and markets led to the emergence of the colonial system.

New technologies were transplanted to the colonial areas in a very one-sided manner. For example, on the Indian subcontinent, the development of a network of railroads and the construction of irrigation systems served to greatly decrease the incidence of famine, with the result that population increased substantially. But at no time in most of these areas did increased production exceed increased population to any marked extent. As a result, the lot of the average individual improved with time only very slowly, if at all.

Following World War II the process of fission accelerated, in part as the result of decreasing rates of population growth in the richer countries and increasing rates of population growth in the poorer ones. The process of fission has now reached the point where most people in the world are either very rich or very poor, and not very many people lie in the middle. With each passing year the gap between the rich and the poor nations increases.

The culture of affluence is now made up of some 1,000 million persons having an average per capita GNP of about $2,300. The culture of poverty is made up of some 2,500 million persons having a per capita GNP of about $180, or thirteen fold less than the characteristic of the rich countries. Within the rich countries per capita GNP is increasing about 4 percent yearly; within the poor countries the lot of the average person is improving much more slowly—about 2 percent per year.

As an illustration of the impossible situation which would develop were present growth rates to continue, 130 years would be required for the poorer countries to reach the level of per capita income which

is now characteristic of the richer countries. But by that time, at present growth rates, the population of the poorer countries would reach 150 billion persons. At the same time the per capita GNP's of the richer countries would approach $1 million per person.

Clearly this is a process which cannot continue indefinitely. Almost certainly the world is slated to experience dramatic, even explosive, changes within the next 2 or 3 decades. Modern technology has linked the peoples of the world into a vast communications network. The inhabitants of even the most remote Andean villages listen to transistor radios and an increasing proportion of the world's population has at least occasional access to television. Motion pictures are shown worldwide in even the smallest villages. The poor see how the rich live and would like to share some of the affluence. Desires for more rapid development are intensified, and whereas a few decades ago the poor might have been more or less content with their lot, today they are impatient and are increasingly willing to use strong, even violent means to help attain their goals. Even in the United States we have seen this phenomenon take place among the poor in our cities. It is a process which is rapidly becoming global, eventually to embrace two-thirds of the human population.

We must add the fact that modern industrial societies, as compared with peasant-village societies, are extremely vulnerable to disruption. We have seen in Indochina how very difficult it is to subdue a determined peasantry even using as we do a broad spectrum of technologically sophisticated weapons. By contrast we have seen how effective the techniques of hijacking, kidnapping, and terrorism can be in industrial societies. We have seen large areas blacked out by accidental power failures and many of us, I am sure, have wondered about the potential effect of one or two well-placed bombs in one or two well-selected power stations in the eastern part of the United States in the summer.

The richer nations can of course devise increasingly sophisticated techniques aimed at keeping the poor countries from getting too much out of hand. But such techniques are more often than not rendered ineffective by the fact that the rich countries fight among themselves, compete for resources and trade, solicit allies among the poor countries and provide them with massive quantities of armaments. Total military expenditures by the poorer countries 1969 came to $26 billion. This exceeded by $8 billion the total expenditure by those countries for public education and health.

I suspect that unless development takes place in the future at a rate which is considerably more rapid than at present, the poor countries in their misery will erupt and that the rich countries in their stupidity will take sides. We see this happening today in several part sof the world and we may well see such developments become widespread in

he years ahead. This process could lead to a global war which would lrag all of us down, or it could lead to a more or less permanent livision of the world into two groups: the rich minority and the niserable majority.

Rapid economic development of the poor countries is essential if we are to create a world in which people world wide can lead lives ree of the fears of starvation, disease, deprivation, and war. But I am convinced that such a world will not come into existence unless he rich countries take the development business far more seriously n the future than we have in the past. At this stage it is extremely loubtful that the poor countries can develop by themselves at rates which are truly commensurate with the need. Almost without exception they need a great deal of capital and technical assistance from the outside.

The process of development involves an enormously complex array of activities. Resources must be surveyed, analyzed, and developed; factories, railroads, and highways must be built; in the public sector housing must be developed, educational systems must be planned, teachers and doctors must be trained; in the agricultural sector irrigation systems must be built, varieties of new seeds must be developed, fertilizers and pesticides must be manufactured and delivered to the crops, farmers must be taught new techniques. All of these activities, which are related directly and indirectly to each other, must be carefully planned and executed as parts of a broad development plan. All of these activities require money, and all require cadres of trained people.

Over the years planners have tended to use what is sometimes referred to as the "black box" approach to development. Here it is assumed that the primary insufficiency in the poorer countries is money and that if only we were to put enough money into one end of a black box called the "development process," development would automatically flow out of the other end. We now have a great deal of evidence which tells us that this is not correct—that although money is an essential element of development, it is by no means sufficient. Other ingredients must be added, some of which we have come to appreciate only in recent years.

Studies of the economic development of the United States tell us that our own development cannot be explained satisfactorily solely on the basis of the classical economic ingredients of land, labor, and capital. Something else has made a contribution which has been fully as important as the three classical ingredients combined, and that "something else" appears to be a mixture of such elements as innovation, research and development, education, training, and improved management practices. All of these factors have clearly been involved in the amazingly rapid growth of our agricultural productivity. It

has been estimated that the return on the investment in agricultural research, development, and education over the past 100 years in the United States has averaged something like 100 percent annually. Such a large dividend (which we did not realize we were reaping at the time), suggests that we did not invest enough effort in such activities.

In recent years my own experiences and observations in a number of developing countries have led me to conclude that expanded transfer of capital from the rich countries to the poor are essential if development is to be accelerated. It is doubtful, however, that a really major increase in capital flow (a factor of two, for example) could be effectively absorbed at the present time for the reason that there simply are not enough trained persons in the poorer countries who are able to make the decisions which must be made and to solve the problems which must be solved if development is to take place. Nor is there adequate organizational structure which would permit decisions to be transformed effectively into action or which would permit development problems to be solved systematically. Nor are there adequate numbers of technically trained persons who can carry out the multiplicity of tasks which are essential in even a quasi-technological society. Indeed, this appears to be a really basic limiting factor to the rate of development.

My first conclusion is that the governments of the richer countries should transfer to the poorer ones as much capital each year as can truly be absorbed effectively. To start, this would amount to significantly more than present levels of transfer, but not overwhelmingly so, and migh' within a few years build up on the average to something like one percent of the GNP's of the donor countries. This is hardly a level of transfer designed to bankrupt the richer countries, but handled properly it could have a profound effect upon development rates. Were the rich countries today to transfer 1 percent of their GNP's to the poor countries, this would amount to 5 percent of GNP's of the developing nations. It would correspond to about 10 percent of what the rich countries are now spending on armaments and to about 60 percent of the annual direct cost of the Vietnam tragedy to the United States alone. We have now traveled along the military pathway for many years and it has failed to lead humanity to its goal. It is now time for us to travel along the pathway of compassion.

Equally important, however, massive technical assistance programs should be created, of far greater magnitude than any such programs attempted before. These programs should be aimed at producing decisionmakers, problem solvers, managers, and other technical persons, at developing the organizational framework within which such persons can operate effectively, and also at devising innovative solutions to specific development problems and bottlenecks.

What should be the basic components of an expanded world program of technical assistance? Here we can obtain some concept of the needs if we trace the implications of programs which are aimed at the primary objective of producing adequate quantities of food.

In the greater part of the developing world agricultural productivity is low both in terms of production per man-hour and production per acre. The effectiveness with which solar energy is transformed into food energy in wheat, for example, is usually limited by the availability of plant nutrients such as nitrogen, phosphorous, potassium and water. Yet adding such nutrients in quantity is usually either ineffective or deleterious because the plant itself has been empirically selected over generations as one best able to survive and grow in the local impoverished environment. The plant is more often than not unable to cope with a substantial increase in the availability of nutrients.

An obvious approach to the solution of this problem is to breed varieties of plants which can thrive in the local environments and which can effectively utilize large quantities of nutrients. This has been a major objective of the Mexican wheat program operated by the Rockefeller Foundation and the Government of Mexico for over a third of a century and of the International Rice Research Institute in the Philippines. Promising varieties of plants which have been developed by those projects have been transplanted to local environments and further modified so they can grow most effectively under the local conditions.

Once this task has been accomplished, sufficient seed must be produced to permit planting by farmers on a significant scale. Fertilizers must be produced on an adequate scale and distributed. Often supplementary water supplies must be provided either from wells or irrigation canals. Pesticides must be produced and distributed.

Thus, to obtain higher crop yields one must construct factories, locate raw materials, provide sources of energy, build highways and trucks, railroads and trains. In other words, a significant elevation of crop yields necessitates a significant level of industrialization.

As crop yields increase, new problems emerge. One must always be on the lookout for new pests which might get out of control. As output per man-hour increases, less farm labor is needed and the surplus people migrate to the cities. As nutrition improves, infant and child mortality is lowered and the rate of population growth increases. The urban areas, unable to provide jobs for the migrants become inundated by vast slum areas and the local government finds itself unable to expand community services as rapidly as people move in. On the farm increased production strains storage facilities and distribution systems.

Thus the apparent solution of one problem, that of low farm productivity, can trigger as in a chain reaction, a variety of new problems, all of which cry for solution. Further, this one illustration emphasizes how inextricably entwined the problems of a society really are—indeed so much so that one must necessarily consider systems rather than isolated problems out of context. We encounter the same kind of situation no matter what priority problem we consider, no matter whether it be public health, population growth, urbanization or industrialization.

Clearly a major component of technical assistance should be science and technology—all related to technological transfer, to the solution of national development problems, and to the building of the organizational structure which will make this possible. The basic aim of technical assistance should be to help a developing nation select, adapt and develop technologies (physical, biological and social) which will help it achieve its social and economic objectives. Development is not simply a matter of transferring existing knowledge and techniques. No matter what the technology, no matter what the country, no matter what the problem which is to be solved, new problems almost always arise which cannot be solved by remote control. They must be solved locally and the chances are that if they are solved at all they will be solved by local people who know their culture and environment far better than do most outsiders.

Research, analysis and problem solving are major keys to development and should consequently be major elements of technical assistance. Here the United States can play a role which in the long run can far transcend our monetary role. With little question we have the highest technological problem-solving capacity in the world. Properly mobilized and oriented and properly coordinated with the programs designed by our friends and colleagues both in the developing and developed countries, I have little doubt that a revolution (using the word in its best sense) can be brought about.

During the past decade the National Academy of Sciences has developed a number of bilateral programs with sister organizations in developing countries aimed at strengthening their local scientific-technological problem-solving competence. In these programs we have brought together natural scientists, social scientists and engineers in roughly equal numbers from our two countries to discuss the problems of development, with particular reference to the role of science and technology. The quality of the discussions and the actions which have resulted have far exceeded our initial expectations. We have helped our colleagues create research councils and institutes; we have jointly modified educational approaches; we have effected nutrition recommendations; we have developed guidelines for industrial research.

Indeed, as a result of these experiences I am convinced that the scientific-technological community, worldwide, can play a crucial role in the development process in virtually all developing countries.

Here in the United States we have a real problem. How can we best mobilize our own resources (which are considerable) to better expedite development? Beyond that, what should our total level of support be?

The overall foreign assistance program of the United States has been the subject of much study in recent years, culminating in the report by the Peterson Panel to the President in the spring of 1970. Perhaps the most important recommendation of that panel was to separate capital assistance from technical assistance and to channel increasing proportions of our capital assistance through the international agencies such as the World Bank and the Inter-American Development Bank. Technical assistance would be placed in an International Development Institute mentioned yesterday by Secretary Rogers, which President Nixon in his message to Congress on September 15, 1970, indicated he hoped would be created some time in 1971.

The channeling of capital assistance through the international agencies makes a great deal of sense for it decreases the temptation to use foreign assistance as a political weapon.

The creation of an International Development Institute makes a great deal of sense, for properly organized, it can provide the structure which can truly mobilize the technical genius of our country for an expanded, reoriented development effort. Such an institute, again properly organized, can enable natural and social scientists and engineers to work on a people-to-people basis with their colleagues overseas on development problems. It would enable development-oriented research to be undertaken domestically as well as overseas. It would make full use of our specialists in industry, agriculture and our universities. Above all, it would provide for the kind of long-term continuity which is so essential if programs are to be successful. Here we must keep in mind that there is no such thing as instant development—that we are dealing with a time scale of a generation or more. We must also remember a quarter of a century of effort went into the making of the so-called Green Revolution.

In our past technical assistance efforts our programs have been handicapped by short-term outlooks. The yearly budget cycle, the 2-year congressional cycle, the rapid turnover of AID personnel and the fluctuating international political atmosphere have all combined to make the conduct of meaningful long-term programs at best extremely difficult and more often than not impossible. Hopefully the International Development Institute will change that situation.

In what ways might the International Development Institute accelerate the development process?

1. It can develop a sound research base for the solution of the critical problems of development which are now being encountered. I include among these the problems associated with rapid population growth, the need for increased food supplies and better nutrition, the problems of rapid urbanization and associated with it massive unemployment, lagging industrialization and the difficulties of transferring technology. I believe that all of these problems are soluble, given the right kind of imaginative effort.

2. The Institute, working with appropriate groups overseas can help develop local scientific-technological competences by helping to create and strengthen the kinds of local organizations which are necessary and by helping to produce the cadres of trained technical people which are needed. Developing countries must develop their local capacities to identify and work on their own research priorities.

3. The United States can develop the incentives and means to orient an increasing proportion of its total R. & D. effort to development problems. At present, a trivial fraction of our huge R. & D. effort is development oriented in an international sense. We can develop research laboratories for the purpose, and encourage young people to choose development careers. We can help universities, industries and other research institutions gear themselves for effective technical cooperation overseas on a stable basis.

4. Technical cooperation can be made into a truly collaborative venture, completely eliminating the "father-child" approach or the "master-pupil" relationship. We know from sad experience that the United States does not always know best.

5. Technical cooperation can be put on a self-sustaining long-term basis, with continuity of support and programs.

What practical results might come out of such efforts? I can imagine that we might in collaboration with our colleagues overseas develop a number of new approaches to the problems of population growth. I can imagine collaborative development of a variety of high labor-input, low capital-input industries. I can imagine our jointly "mini-industries," suitable for many locations in developing countries. I can imagine the development of a variety of new approaches to transportation problems in the areas, to the problems of low-cost housing, sanitation and public health. I can imagine a rapid expansion of the Green Revolution to include more countries and more crops. I can imagine new educational techniques, new applications of food technology, and the development of new industries which will make maximum use of local resources.

About a year ago the Agency for International Development reorganized its own programs of technical assistance and created a Techni-

cal Assistance Bureau. That Bureau which has a newly formed unit dealing with science and technology, has already demonstrated on a rather small scale what can be done in spite of numerous handicaps. Its experiences suggest that the International Development Institute, properly organized and adequately funded, can revolutionize the development process.

How large need such a program be? Starting out as a present level of about $500 million annually now being devoted to technical assistance, I would visualize that in time it could effectively utilize perhaps about some $2 billion annually on programs at home and overseas both bilaterally and multilaterally. Although this is quite large compared with present programs, the situation is desperate and major actions are called for. And I suspect further that we would receive for our money a "bang-for-the-buck" unprecedented in foreign assistance efforts.

THE ROLE OF SCIENCE POLICY IN SOLVING SOCIAL PROBLEMS

THE UNBALANCED PROGRESS OF PROGRESS

STAFFAN BURENSTAM LINDER[1]

The concept of science policy may be new and fashionable but the problem is as old as science itself. To allocate resources between competing projects and to determine the total size of the scientific effort is a science policy problem which those interested in scientific work have always been exposed to. Columbus, in trying to get funds to pay his way across the ocean, encountered this problem of science policy, if not explicitly so at least implicitly

But, in the old days there was a more deep-going problem in science policy than the money matter. This was the problem of how to choose between truth and dogma when the two collided. If Columbus encountered the menial aspect of science policy Copernicus met the old, fundamental problem of science policy. At that time the struggle between the two sets of ideals must have caused severe social strains and the way the conflict was resolved has had wide social consequence. The results of science have transformed society but already the acceptance of the method of science made deep changes in religious and political life. Once the right of scientists of pursuing the truth has been accepted, the freedoms of thought and speech which we consider basic to our culture are more secure. The strength behind the scientific ideals is shown by the fact that in countries where the liberal freedoms are sometimes denied, few efforts are nonetheless made to tamper with academic freedom. The difficulty of doing the latter probably restrains sinister wishes to do the former. However, even if the threat to the ideals of scientific method have not completely disappeared a[illegible] may present themselves again, this is not what we think of when we nowadays discuss science policy.

Once scientific efforts were not only accepted but unhesitatingly promoted as an engine of progress, science policy did not cause much attention. The money matter, of course, existed but at a time when thought was speculative rather than empirical science was also inexpensive. It is only during the last ten years that science policy has begun to attract wide attention again. The reason for this was first that the costs of research, at least within the natural sciences, rose

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rapidly at the same time as it became more widely recognized that technical progress was the force behind the spectacular economic growth. Options became wider and costs higher. Thus, the problem of promoting research without incurring prohibitive costs and of allocating resources between competing projects became more important.

However, even if money problems form a part of science policy more now than before, the chief reason for the present attention given science policy lies elsewhere. It is a fundamental problem that again has risen although now in a different shape.

Now it is not truth versus dogma but truth versus truth or, more clearly, truth versus abuse of truth.

The problem is not that truth may destroy dogmas but that truth may destroy itself.

That science is no guarantee of happiness may be accepted. And that economic growth built on the results of science and technology is no guarantee of happiness may also be accepted although perhaps more reluctantly. But, what some have come to suspect during recent years is that, actually, the causation runs the opposite way. Science, technology, and economic growth may, instead of not guaranteeing happiness, actually guarantee unhappiness.

This suspicion is of great social consequence. The idea of progress is the basis of our civilization since a number of centuries. It is since the Enlightenment the foundation of our social contract. It offers the hope which is required for the acceptance, the temporary acceptance, of the unavoidable ills of an unfinished society that is built on a principle that does not accept any ills. This is the principle of egalitarianism in the sense of happiness for all. I think that everyone who has been involved in practical politics is aware of the importance of the idea of progress as the escape when, unavoidably, hard choices present themselves.

If the current suspicions are well-founded it may prove that their causes, through the abuse of truth and science, will lead to the destruction of our civilization. The suspicions themselves may cause defeatism and even a reaction against science and technology that may completely change the direction and position of our civilization. There may be a Luddite reaction against research.

These problems are social problems which call for adequate science policies. They require measures of an economic and social kind, too, but for continued progress engineered through further scientific and technological advances these advances must be shaped through science policy in a form which prevents social strains.

In order to see more clearly what is required it is interesting to analyze the reasons for the present disbelief in science. I think that it is possible to arrange such an analysis around the theme that *the problem of progress is that progress has progressed in an unbalanced way.* I shall point at five different kinds of such unbalances.

To begin with I shall repeat the often made observation that there is an unbalance between the progress of science and of man. There is no particular reason why the scientific efforts should have resulted in any moral advancement. However, for the results of science not to be abused, such an advancement is called for. Presently, it is not magnimity and greatness but fear that provides uncertain checks. Even if that fear is needed for restraint, it is a grinding feeling to live with.

Secondly, there is an unbalance between technological progress and the capacity of man to adjust to the demands of the new technology. New techniques of production are intended to be our servant but sometimes they become our master. For the utilization of new techniques certain capacities not available to all are required. For instance various statistics indicate that life in the big urban centers expose people to unexpected hardships.

In modern society there seems to be growing minority groups which in one way or another have failed to adjust to the new demands. To this category belong criminals, narcotics, alcoholics and people otherwise socially and mentally handicapped. These minorities constitute serious social problems, class problems of a non-marxian kind. Their situation is aggravated by the fact that costs for rehabilitation and care are rising with rising wages, these services being highly labor intensive requiring skilled labor. The minorities of this kind are exposed to a new kind of poverty the backgrounds of which is not a low level of productivity but inability both to adjust to the demands of a high productivity system and to obtain adequate services in such an economy.

Against the background of rising service costs we can also see the increasing difficulties for medical care and old age care.

The third and fourth type of unbalance also concerns new types of poverty caused by economic growth. There is a dangerous misunderstanding that economic growth leads to general affluence. It only leads to partial affluence. It is the quantity of goods that increases but many other ingredients in a good life do no become more plentiful. One important example of this is that time at our disposal for various activities together with our consumption goods does not increase. We only have our 24 hours per day at our disposal. When, as a result of economic growth, the quantity of goods increases whereas the amount of time is fixed, time becomes more scarce in relation to goods. Time becomes more expensive. It must be more carefully husbanded. We all know how this is done in production where more capital goods are combined with each hour of labor to yield a higher productivity. The same happens in consumption. Greater quantities of goods are combined with each hour of leisure to yield a higher level of satisfaction. Since activities differ as to the ease with which it is possible in this way to increase the pleasure associated with them, there will be a re-

allocation of time among the various activities. Some will expand, others will decline. Activities which it is easy to make more pleasura with an additional amount of consumption goods per unit of time will expand, and vice versa. This reallocation will rise to important social changes, changes which at least some commentors would call problematic. Most of those activities which the original economic philosophers like John Stuart Mill considered especially desirable and which they hoped and thought that economic growth would stimulate are activities, the pleasures of which cannot easily be expanded with bigger and bigger injections of consumption goods. To these activities belong reflection and meditation, most of the cultural activities, friendship and love, peace and quiet.

It is already pointed out the increasing scarcity of time which makes if difficult to solve the rehabilitation problems of the minorities which have been unable to adjust to the high productivity economy. It may also be that the strains of the hecticness of modern life is one reason why some people have difficulties of adjusting and fall out into the minorities.

The fourth unbalance is perhaps the most dangerous example of the new poverty. Economic growth leads to an affluence of goods but to a scarcity of natural resources like air and water. The supply of natural resources decreases and the demand for natural resources increases as a result of production and consumption. When, through economic growth, production and consumption actually increase, the scarcity of natural resources becomes even more pronounced. As a result of this unbalance we face all the environmental problems, the threat of which causes serious social problems. There may be an erosion not only of the soul but also of the spirit.

A fifth unbalance is caused by the uneven geographic spread of the benefits of modern science and technology. They have not reached the underdeveloped countries and the depressed areas and poor regions in the rich countries. Thus, it is not only the new forms of poverty but also traditional poverty that causes social strains.

This is no complete catalogue of the social problems caused by the unbalanced progress of progress. It is an analysis of some of the mechanisms and the resulting troubles, which, in the judgement of many, have lowered the prestige that science deserves for its great contributions. To help to correct the unbalances we require a science policy. Thus, science policy again takes on the importance it originally had but which it lost when the conflict between truth and dogma had been settled. There are three main strategies that may be pursued:

(*a*) Continuation of present policies.

(*b*) Prevention of scientific and technological progress.

(*c*) Adoption of a cautious, but well financed program of technological development.

A continuation of present policies is dangerous. The unbalances are not—as could be hoped—self-correcting in a way that unbalances in the market system are. At least the self-correcting forces are not working fast enough to be relied on. There are no supply-demand forces operating smoothly through a price mechanism to correct unbalances as had an invisible hand been active. In science policy the hand must be visible.

The second strategy—to let the hand stop the clock of progress—has its advocates at least within economics where there has been some talk of the "no-growth-economy". It may also be noted that funds for science during the last few years have stagnated or even declined. However, to accept a science policy which would yield a "no-science-society" would not be acceptable. It would leave uncorrected some of the unbalances which exist. It would mean that other unbalances would, actually, increase. The dangerous new poverty in the form of environmental problems would deteriorate as even a "no-growth-economy" would lead to a continued wearing down of natural resources. Finally, to accept suddenly a no-progress-society would represent a dramatic change in our basic attitudes and aspirations, a change, for which we are not prepared. It would probably cause severe political strain as so many political conflicts are solved through promises of betterment and outward signs that the promises can be at least partly fulfilled.

Instead, our conclusion must be that we require science, and require it badly, to solve the problems that the application of science has caused. However, this science must not result in new problems bigger than those solved. There is in the progress of science the inner elements of tragedy: science has created forces that can be controlled only through more science but this new science may impose its own threats. To avoid a final tragedy we need a science policy and that science policy must follow the third strategy of a cautious, but well-financed progress of technological development. Such a science policy can be divided into four parts.

(A) DETERMINATION OF THE TOTAL R. & D. EFFORT AND ALLOCATION OF FUNDS BETWEEN COMPETING PROJECTS

This is the traditional resource problem which has become more pressing due to the wider options and higher costs. There is a need for policies to manage science as such both within a country and through international cooperation to get the highest possible returns. However, in the final analysis other considerations must now enter. There must, as we have seen, be an evaluation of the wider social consequences. We require what we can call a "management of insights" on the following three levels influencing the final allocation of resources.

(B) REGULATION

As a result of the dangerous side effects of many of the results of science it is important, as part of a science policy, to determine if and how these results can be controlled. The control of atomic power both in a military and civilian context may be mentioned as an example. The possibilities of exerting such control must be taken into account in determining whether to promote or not promote technological development in particular fields.

(C) FORECASTING

Through technological forecasting which is part of the modern future studies it is possible to make informed guesses of prospects and problems. Such forecasting can improve the quality of the allocation of funds between competing projects. It can do this especially by serving as the starting point for technological assessment.

(D) ASSESSMENT

This is the part of science policy which is the most discussed recently. Assessment goes beyond mere forecasting in trying to evaluate systematically the advantages and the risks of new technologies. The assessment can be either of a certain technique or of the possibilities of solving a certain problem through alternative strategies using different technologies.

The analysis of the various unbalances of progress serves not only to illustrate the need for a science policy but also to facilitate comment both on the possibilities and on the limitations of science policy.

There are certainly possibilities for science policy to help us to remain within a context of real progress. It should be possible to find ways of relieving the problem of traditional poverty, underdevelopment, due to an uneven spread of technology. Through a determined effort technologies suitable for the conditions of underdeveloped countries could certainly be promoted. The underdeveloped countries form what has been called a "research desert" and since our economies have blossomed through technology, their economies are unlikely to grow sufficiently fast in such a desert.

Similarly, it would be surprising if important results could not be reached through a careful assessment both of new technology from the point of view of its ecological consequences and of environmental problems, and alternative technologies to handle them. It would be surprising since so little has been done in this direction and since in this case technologies interact with nature rather than with the human soul. Thus, it should be possible to advance through a utilization of the methods and arguments of the natural sciences themselves. In view

of the seriousness of the environmental problems science policy would be called for even if it could not do anything more than to relieve these pressures and the social strains that follow from a powerless watching of the thickening clouds.

But that there are limitations of science policy should not be overlooked in the present enthusiasm for such policies. Science policy cannot do much to correct the unbalance and pressure which arise when belligerent and antagonistic man is better and better equipped for annihilation and only grinding fear can provide checks. Hopefully, peace and conflict research in an assessment could be found to provide insights into how a more enjoyable foundation for peace than fear could be found. Some consolation could also be found from the fact that the superpowers in an evaluation have reached the conclusion that some defense research is associated with dangers for all parties concerned. Thus, they have shown signs of stopping such research. Such a sign is the U.S. declaration of not wanting to utilize biological weapons even for defense. Research on such weapons carry the risk that methods for their production could become simple enough for a multitude of countries to learn them. However, on the whole, it is rather a change of heart than a change in science policy that is required to correct the unbalance between technology and the inner qualities of man.

A change in values rather than in science policy would also be required for more people to avoid a stressing pattern of life where economic growth is permitted to result in a hectic utilization of time in commodity intensive activities. No science policy can correct this kind of imbalance even if scientific explorations of modern ways of living to more people may reveal some of its futilities.

Opponents to science policy may argue that not only does such a policy leave many problems unsolved but it creates new problems. There may, in particular, be a fear that science policy would mean a manipulation for political purposes. However, modern science policy is not concerned with the old problem of the conflict between truth and dogma. In fact, science policy may be required to avoid political manipulation through the results of technology. The risk presently is not so much that certain projects will be stopped for partisan purposes but that certain projects will be promoted for such ends. A science policy could permit a more open debate on the disadvantages and merits of the policies chosen.

To reduce the risks for political abuse basic research should not be exposed to more than unavoidable allocation process of scarce resources. Furthermore, technological assessment need not be applied to the social sciences. Social research hardly results in new technologies of a dangerous sort. Thus, there is no need for control and assessment of its result.

Writings within the social sciences may instead be politically controversial and a system for interference could be abused. Furthermore, research within the social sciences is inexpensive which means that careful evaluation for appropriate allocation of money is not as necessary.

Finally, a limitation on science policy is that it is easy to say but difficult to make. Dr. Harvey Brooks has rightly pointed out that "many of the current demands for better scientific planning are probably as naive as the early demands for economic planning".

One limitation exists for small countries. As in the economic context they will be highly dependent on what happens elsewhere. They have difficulties in making a wise allocation of resources in isolation. They do not have resources to make all around technological assessments. Technologies developed elsewhere become, as a rule, part of their system without any particular evaluation. For the small countries to pursue a science policy it is necessary to engage in international cooperation. Only the US has resources wide enough to rely on a national science policy. However, even the US must in many instances fall back on international cooperation and will be influenced by actions taken in other countries. One field where world wide cooperation is required is in the control of the environmental hazards.

A limitation of a different kind arises out of uncertainties. No matter how carefully science policy evaluations are made they must be carried out against the background of great uncertainties about the future. But, there are also uncertainties in the present. Through improved methods for cost-benefit analysis it may be possible to base decisions on firmer ground. Yet, it is extremely difficult to attach weights to the advantages of competing technologies and strategies.

The same observation could perhaps be made with respect to most political decisions. However, correct decisions in science policy requires more expertise than is generally found among parliamentarians who must have many other strings on their bows. The more complicated the world and the more important the science policies, the more important that politicians shift some of their interest over to an understanding of the technical complexities and that they are assisted in so doing through institutional arrangements facilitating their contact with technological problems.

The uncertain situation of politicians in the formation of wise science policy is a difficulty. An even greater difficulty arises when and if attempts are made to introduce a greater element of direct democracy in the formation of science policy. This is a dilemma. The difficulty of the lay man to participate in science policy discussions is equal to the necessity of widening this debate. Otherwise it may be impossible to obtain a political basis for many decisions that must be taken. Immediate small benefits may in the popular debate loom larger than

long run imperatives. In various ways—for instance, through an improvement of general education—such difficulties may be reduced. However, their unavoidable existence puts an even greater responsibility on politicians to show leadership and demonstrate political will in the search for wise science policies.

Against this background the Committee on Science and Astronautics is to be praised for its many important initiatives in stimulating debate of science policy matters.

INTERNATIONAL SCIENCE POLICY IN THE MARINE ENVIRONMENT

JACQUES YVES COUSTEAU [1]

I have been asked to express my views about international science policy as we have observed it in the marine environment. I will start by outlining the deep concern that is felt by observers like ourselves about our environment.

Looking at our little endangered planet from out in space as the Apollo crewmen did, I will try to review in that new perspective very briefly the goals of environmental science and finally outline a practical emergency plan.

The news, as you all know, brings to us every day additional reasons to fear about our environment, an mainly from the sea, because the sea is the universal sewer where all kinds of pollution end up conveyed by rain from the atmosphere and from the mainland. Large oil tankers collide, sink, burn. Lakes and rivers are poisoned forever. Coral reefs die rapidly.

All other the world automobile exhaust turns the air of our proud cities into a fluid dangerous to breathe. The lead content of all oceans has increased 500 percent since 1933. Pesticides soften the shells of seabird eggs. Shipments of canned crab are found radioactive. Tuna fish is poisoned from mercury. People get hepatitis from swimming in the North Sea. There is now not one single beach which is not polluted by oil and tar. I can add many hundreds of such statements that are all easy to verify.

In our diving crew, the veterans like Falco, Gemma, and myself are very special witnesses of what is going on in the sea. For more than 20 years we have dived in all places around the world. We have tried to estimate—and we are fully aware that this is no scientific measurement—the decrease of all population, in the open ocean as well as near shore and near distant islands. During these 20 years in the places that we were lucky enough to visit twice, our rough estimate is that the decrease of population and the vitality of the ocean is down more than 40 percent in 20 years—it may be 35, it may be 45; it is that order of magnitude.

The primary production of the sea may not be affected to that same amount. We have not been able to measure the primary production

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for long enough to be sure. The special concern of the environmentalist is that the destruction spreads in what appears to be an exponential mode. The only reason for hope at the moment is the growing awareness of the public, especially in the United States and in Europe.

A survey that I made from the tens of thousands of letters that I have received, proved that adult men above 30 years of age do not worry as much as housewives who fear for the very future of their children, or as much as young people who have today access to much more information than the adults have.

Two years ago a decisive event made a complete revolution in the thinking of every man. For the first time our planet was contemplated by human beings from outer space. It is a cliche to repeat it, but we cannot avoid it because it is decisive.

I met in Paris the heroes who had been first to walk on the moon. What they remember mainly from the moon is how beautiful the earth is. Rare and beautiful, because it is the only planet in our solar system to be attributed with appreciable quantities of water. Overnight, all living humans realize it.

Yes, earth supported life only because it had water. Water and life are inseparable. Poisoning water is an assassination. It would eradicate all life on earth. This is what everybody realized in Africa and Asia as well as in America.

It is not possible to study a phenomenon without relating it to its global mood. That is really what the Apollo revolution is about. Men were invaded by a frightening feeling of isolation in space. At the same time, they felt an urge for solidarity. Today's youth feel lonely and grope more than we did when we were young, because isolation generates the need to get honest comprehension and communication.

Apollo's influence is to be felt in everybody's everday life and in science as well. Moon rocks and rocks from the bottom of the sea collected by the ship *Glomar Challenger* tell the story of the solar system. Airborne and aerospace oceanography broaden the understanding of the ocean, which was pretty narrow when we only had ships to study it.

Scientists compute global quantities of carbon involved in the life cycles, in carbonates, or in fossil fuels yet to be discovered, with an incredible precision. Frightening news came from biologists recently who have now an idea of total living organisms compatible with conditions on earth and measure the very slow rate of exchanges possible between the main masses of the biosphere. Sea life between the surface and a thousand feet in the open oceans is practically isolated from the rest of the biosphere, with extremely slow exchanges. These are the latest discoveries, and it means that preying on our capital in the open ocean depletes the stock with no possibility to put it back before millions of years.

Oceanographic measurement must now be obtained worldwide simultaneously to have meaning—the product of a giant network of photometric studies to be implemented by airplanes by the tens of thousands, keeping us informed better than the meteorology stations.

Scientists are working toward such global projects as weather control or even flattening the sea. Because of the political, sociological, and philosophical consequences of Apollo's vision of our planet, I would say that the very basic goals of civilization are being reevaluated.

Production of goods and competition for this production are no more goals in themselves; but if properly redirected, industrial production will be more indispensable than ever because the happiness of man is not what activities he aims at, but what he should aim at.

Here I open a paradox: The environmental alarm is used widely by extremists for subversive aims. We should be very cautious not to fall into two of their traps. One would be to react and say, if the subversive people are using this, we are to discard the entire subject. We also should avoid the attacks that these people are aiming at our industrial Western civilization.

In between there is a reasonable equilibrium balance to keep. In order to enable mankind to carry on its course in the pursuit of happiness, the essential needs are obviously the elimination of hunger and of the basic needs for all men, the elimination of disasters, better health, increase in life expectancy, enjoyable peace, raising the intellectual level of average activities of leisure, and mainly promoting cleanliness and beauty of man's environment.

It is toward this new perspective of the world that environmental sciences are now progressing. Worldwide, as you know, there is a very general shift from the basic, uncoordinated science that was born in the small laboratories under individual initiative, toward oriented sciences performed by large teams and programed with a small degree of freedom in its research.

Many of the scientists are sorry about it, but that is the way it is. Less money is given by all governments for uncoordinated science, while more money is given for applied oriented sciencie. This is because today the role of science is just to make things possible, not to decide the policy.

Before I outline the plan that I proposed at a Council of Europe 2 months ago, I would like to recall briefly the few principles on which this proposal was made. I said a moment ago that there was a quasi-identity between water and the life cycle. There is also evidence that all pollution ends up in the oceans, whether it is atmospheric pollution, land pollution, or sea pollution. So it is obvious that the only way to get rid of the pollution which exists is to isolate all the toxic products from our effluents in the water cycle.

This isolation may mean to dehydrate or to isolate, but it has to be isolated from the water cycle. If we do so, if we reach that goal, we are saving literally the possibilities of survival.

The amount of pollution that is produced by mankind is of course a complex function of industrial development and of population. The industrial development can be curbed a great deal. The more difficult thing will be to control population. Many of my associates and friends consider the population explosion as the superpollutant.

Also we must admit that the scientific knowledge that is available concerning pollution is very small. Every day a new pollutant is discovered. Every day some concerns are modified. Two years ago there was no concern at all about mercury pollution. Today it is a big thing. What is tomorrow going to bring? This shows our ignorance in this field.

Something that is a guideline for action is that whenever possible, persuasion is preferable to coercion. There is also a basic principle in the control of pollution. It is that this control be officially given to private, independent agencies.

For what reasons? Well, the world over—I am not talking about any country at the moment, but the world over, governments are among the biggest polluters, and today they control themselves. I will give you a concrete example, because that I have the right to say. When my country explodes an H-bomb in the Pacific and says it is less harmful than to wear a wristwatch, everybody smiles; but it is tragic, because it doesn't mean anything. It is lying.

When a big polluter pays for a full ad in the newspaper to explain that their own anaylsis shows that their fumes and effluents are harmless, it is obvious what they print is right. But they don't print everything. Accordingly, it is a basic principle of justice that you cannot be at the same time judge and be judged.

There is no way to get the truth to the people except in giving the controls to independent, private agencies. This is done for ship running. In England it is Lloyd's; here I don't know what it is. They are private companies that have acquired a semiofficial position and that are trusted by everybody.

This is the way we should go for control of pollution the world around. I spoke a moment ago about persuasion. I would emphasize the role and the importance of the young people. Today, as you know, when children come back home, in most of the cases they know more than the parents do and they teach their parents. Today in the homes, the education of the adults is done by the kids. The kids get their information at school, from television, from books. It is a paradox, but it is true.

The teachers' associations have a great influence They are all concerned with the environment. They teach the ba... facts to the

kids and the kids in turn introduce these facts to the homes, so this is the circuit by which persuasion can be spread.

About overpopulation, we do not know exactly the law that correlates pollution with industrial development and number of people. It is obvious that it is a function of both. If we were able to measure pollution, and if we are able to determine the total amount of pollution that the earth can stand without definite damage, then we could draw the maximum number of people the earth is capable of supporting.

Obviously this number would decrease regularly with the increase in industrialization. So the advisable pattern for population is a progressively decreasing pattern whereas it is now an exponentially increasing pattern. I am making this remark because I have no solution for this problem. It is the biggest one. As long as it is so, the rest will be of little account.

Nevertheless, let us outline a program of action. We have estimated that the amount of money allocated to the sciences of pollution and environmental sciences should be multiplied by a factor of 20—minimum. Results will be slow to come, even at that pace. So action must be taken before results are obtained and corrected according to the new knowledge acquired. But it would be a fantastic mistake to wait for the scientific results before taking action.

That is the first point of the program. The second point is to educate the public, the buyers, and the consumers, not only to explain to them what is going on, but to give them practical ways to fight and to influence the producers by their demand. The consumers are the key to everything. So this education, instead of being subversive, should be organized officially and approved.

The third point is the persuasion of the polluters that profit can be made through the increase of business volume that will be brought about by the fight against pollution. Protection of the environment is another one that should be considered that way by industry. Accordingly, the cost of protecting the environment should be automatically included into the cost of production, the same as raw materials, salaries, overhead, investments, taxes, et cetera.

It has been computed that the amount of increase of the gross national product or of the price in general, would be about 5 or 6 percent of today's prices, which means that in a trillion-dollar economy, the cost for the economy would be $50 to $60 billion a year. It is not impossible, but it is way beyond the means of Government. The solution is not within the Federal budget. It can only be found at the level of the economy. In order to add a little discipline to the persuasion and to the education, there must be regulations. These regulations must be intelligent, comprehensive, and mainly they must be national and international.

But there is a trap. When you speak of international action, everybody can visualize immediately the slowness and the impossibility for the following reason. The biggest polluters are the nations that have the highest income per capita. They are also the only ones that can fight pollution.

The other nations do not have the financial possibility to avoid, for example, certain pesticides. They cannot help it. They do not have the possibility to include the protection of their environment in their gross national product, but they pollute far less than the richer nations. If we wanted to go through regulations from the United Nations, we would never obtain anything.

What I have proposed is that the main polluters get together and go into action. These main polluters are Japan, North America, Europe, and Russia. If these groups of countries get together independently from the United Nations to adopt a policy, it will immediately bring down pollution by about 80 percent. From there on, every country could join later and individually according to their development. These are the simple lines that could be adopted fairly quickly, provided that a strong will is expressed by the people.

I will finish by saying that with the image that already has been described by some people in NASA, I think, of earth being a spaceship, then the oceans are the only water supply of earth's astronauts. Assuming that this is true, and I join it, then I would like to add something: all the problems to save the environment and to make the future of our children possible is subject to lasting peace. When we look at the earth from the moon, as the Apollo commander did and if we consider our earth as a spaceship and the earthly astronauts as the crew of that spaceship, I would say wars can be analogous to mutinies aboard the ship.

POTENTIAL CONSEQUENCES OF EXPERIMENTATION WITH HUMAN EGGS

JAMES D. WATSON [1]

Several years ago a most remarkable frog grew up in Oxford. Its origin did not lay in the union of a haploid sperm cell with a haploid egg, the fertilization process which ordinarily gives each higher animal a mixture of paternal and maternal genes. Instead this frog arose from an enucleated egg, into which had been inserted a diploid nucleus from the intestinal cell of an adult frog. Microsurgical removal of the maternal nucleus from this egg had denuded it of any genetic material. But by subsequently gaining a diploid nucleus (as opposed to the haploid form found in a sperm) the egg acquired the chromosome number normally present in a fertilized egg. As such it could be activated to divide, thereby setting into motion the successive embryological stages which culminate in an adult frog.

The genetic origin of this frog was thus very different from that of all previous frogs, one half of whose chromosomes came from the male parent through the sperm, the other half from the female parent which produced the egg. Normal fertilization processes by combining genetic material from two different parents always generate progeny uniquely different from either parent. In contrast, the Oxford frog derived *all* its genetic material from the individual whose intestinal cell was used as the nuclear donor. The genetic complement of all its diploid somatic cells (as opposed to its haploid sex cells) was thus identical to that in the donor frog. So, in effect, it was an identical twin of the donor frog born some months before. Furthermore, since every adult frog contains millions of cells capable of being used as nuclear sources, the original donor could have served as the genetic parent of thousands of progeny identical to itself.

This type of reproduction is generally referred to as a *clonal* reproduction. (A clone is the aggregate of the asexually produced progeny of a single cell; for example, all the descendants of a single bacteria present as a colony upon a petri dish.) The genetic identicalness of all members of a clone arises from the fact that the normal process of cell division (called mitosis) produces two daughter cells with identical chromosomal complements. The nuclei of the cells found in the frog's intestine are thus identical to those which could be found say in its liver or brain.

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In contrast, the cell division process, called meiosis, which generates the sex cells reduces the chromosome number in half. Only one of each pair of homologous chromosomes enters a sperm or egg. Moreover, a completely random event determines whether the given chromosome is of male or of female origin. So no two eggs (or sperm) arising in a given individual are ever genetically equivalent. No two sexually produced frogs, having the same two parents, thus will be identical unless they arise by the rare splitting of an already divided fertilized egg into two separate daughter cells, each of which goes on to develop into a complete embryo. (This is the process by which identical human twins are produced.) In contrast, all the members of a clone produced by mitosis will be identical, except for the occasional mutant cell resulting from rarely occurring somatic gene mutations.

The existence of the first clonal frog, the result of the work of the English zoologist John Gurdon, was a very important scientific event. He settled the long controversial biological dilemma of whether the process of cell differentiation in frogs was primarily a cytoplasmic or nuclear event. During embryological development, the progeny cells which result from the cell divisions commencing after fertilization become changed (differentiated) into a variety of morphologically and functionally different cell types. For example, muscle cells, nerve cells, and skin cells of a single individual all have a common ancestor in one fertilized egg. Most differentiated animal cells, when isolated from contact with other cells, continue to divide and maintain their specific differentiated state. This fact posed the question of the nature of the factors which maintain the specific form of a given differentiated cell. In particular, does differentiation occur through irreversible changes in the nucleus which somehow alter its chromosomal makeup, perhaps by the mutation of specific genes?

Gurdon's clonal frog cleanly settled this point by showing that a nucleus taken from a highly differentiated cell still retains its capacity for directing the development of a completely normal frog. Differentiation thus does not involve gene mutations. Instead it must be based upon complicated interactions between the nucleus and cytoplasm which effectively command certain genes to produce the specific gene products needed for a given differentiated state. But as soon as one such specialized nucleus is removed from its given cytoplasmic environment, for example by microsurgical introduction into a new cytoplasmic environment, the instruction which its genes receive are no longer the same and a new set of genes will go into action. In particular, when the nucleus of a differentiated frog cell is placed inside an enucleated unfertilized egg it quickly sets into motion the successive steps of embryological development leading first to the tadpole stage and finally to an adult frog.

The question of course arises, will this same basic principle hold for the large majority of differentiated cells? Now I suspect most biolo-

gists will guess yes. In general, very fundamental phenomena, of which differentiation is one, do not have a different molecular basis from one organism to another. Moreover, it is already clear that differentiation in several plant species does not involve irreversible nuclear changes. Now it is routinely possible to produce mature plants starting from highly specialized somatic cells of diploid chromosome number. For example, mature carrot plants can be produced from single callus cells that are placed in proper nutritional environments. Thus it is highly likely that the embryological development of most higher animals, including man, involves the creation of countless numbers of somatic nuclei each capable of serving as the complete genetic material for a new organism. This means that, *theoretically*, all forms of higher animal life may in effect be capable of clonal reproduction.

If true, this situation could have very startling consequences as to the nature of human life, a fact soon appreciated by many magazine editors, one who commissioned a cover with multiple copies of Ringo Starr, another who gave us overblown multiple likenesses of the current sex goddess Raquel Welch. It takes little imagination to perceive that different people will have highly different fantasies, perhaps with some imagining the existence of countless people with the features of Picasso or Frank Sinatra or Walt Frazier or Doris Day. And would monarchs like the Shah of Iran, knowing they might never be able to have a normal male heir, consider the possibility of having a son whose genetic constitution would be identical to their own?

Clearly even more bizarre possibilities can be thought of, and so we might have expected that many biologists, particularly those whose work impinges upon this possibility, would seriously ponder its implications, and begin a dialogue which would educate the world's citizens and offer suggestions which our legislative bodies might consider in framing national science policies. On the whole, however, this is not at all what has happened. Though a number of scientific papers devoted to the problem of genetic engineering have casually mentioned that clonal reproduction may someday be with us, the discussion to which I am party, have been so vague and devoid of meaningful time estimates as to be virtually soporific.

Does this effective silence imply a conspiracy to keep the general public unaware of a potential threat to their basic ways of life? Could it be motivated by fear that the general reaction will be a further damning of all science and thereby decreasing even more the limited money available for pure research? Or does it merely tell us that most scientists do live such an ivory tower existence that they are capable of rationally thinking only about pure science, dismissing more practical matters as subjects for the lawyers, students, clergy, and politicians to face up to in a real way?

One or both of these possibilities may explain why the occasional scientist has not taken cloning before the public. The main reason, however, I suspect, is that the prospect to most biologists looks too remote and chancy—not worthy of immediate attention when other matters, like nuclear weapon overproliferation and pesticide and auto-exhaust pollutions, present society with immediate threats to its orderly continuation. Though scientists as a group are the most future-oriented of all professions (some investment bankers would probably disagree) there are few of us who concentrate on events unlikely to become reality within the next decade or two.

Behind the general belief that the development of techniques for cloning any mammal, including man, lies far in the future, are fundamental differences in the embryological development of mammals and amphibians like the frog. These differences reflect the very different environments in which amphibian and mammalian embryos develop. All the frog's embryological development, even in the beginning fertilization stages, occurs, in-vitro, outside the body of the female parent, generally in the nutrient-poor environment of fresh water lakes and ponds. Thus all the food supply necessary for growth to a developmental stage capable of independent feeding, in the case of a frog to the tadpole stage, must initially be present within the unfertilized egg. As a result, amphibian eggs are not only always relatively large but all their developmental stages are capable of relatively easy experimental investigation.

In great contrast are the eggs of placental bearing mammals. Their eggs are relatively small since they have to contain only the nourishment necessary to reach approximately the 64-cell stage. At this point the tiny embryo implants itself on the wall of the uterus, a placenta forms, and all the food molecules necessary for subsequent embryonic growth comes from the female parent. Not only does the small size of such mammalian eggs make experimentation very difficult, but even more important, all the stages of development normally occur within the ovary, oviduct, or uterus. Moreover, there seems to be no real prospect that any mammal can ever be totally raised in vitro. Thus detailed knowledge about the exact steps in the embryogenesis of any mammal is much, much less complete than that of amphibians, all of whose development normally occurs in vitro.

The cloning of any mammal thus will be far from a routine task. In particular, the techniques of micromanipulation used to insert nuclei into frog's eggs cannot now be applied to eggs in the mammalian size range. They are likely to be irreversibly damaged by the introduction of a nucleus whose diameter is only some two or three times less than that of the egg itself. And if somehow a trick were ever found to successfully insert a diploid nucleus, the equally challenging task of finding conditions for the in vitro growth of the modified egg through

to the adult stage would still lie ahead. Thus the clonal production of human beings has seemed to most geneticists an event so unlikely as to not be worth stirring up public attention.

This assessment would be correct if the pace of research on human reproductive biology would continue at the current rate. With a few exceptions, work on the early developmental processes in man has not been seriously pushed either here in the United States or elsewhere. As a result, there exists a scientific lacuna so serious that it deeply disturbs those people who realistically worry about overpopulation problems. They believe that more basic biological knowledge about human reproductive processes would be very helpful in slowing down the fearful rise in the number of human beings. Consequently, already there is much "population" money available to induce more people to move into the field of reproductive biology, hopefully to learn in great detail the step-by-step processes by which a human egg is ovulated, fertilized, cleaved, and moves down the oviduct to implant on the uterine wall.

A key ingredient to obtaining this information is the development of methods by which the early embryological stages of mammals can be studied in vitro. For as long as study is restricted to work on intact animals, experimental work, as to be distinguished from observational analysis, will be virtually impossible. Most importantly, though unknown even to most biologists, the beginnings of first-rate research on the in-vitro cultivation of mammalian eggs has already occurred. Techniques are in fact available for the isolation of mouse eggs, their fertilization in vitro, and subsequent cultivation under test tube conditions which permit growth to the 64-cell stage. At this point the embryonic body (called a blastocyst) can be surgically implanted back into the uterus of a living mouse, where it can eventually develop to the stage at which normal birth occurs.

This means that most of the techniques that will be needed for a clonal mouse are already available. The only serious obstacles remaining are the development of methods for the removal of the haploid maternal nucleus and the subsequent addition of a diploid adult nucleus. Now there are hints that the enucleation problem will not be serious. For some years it has been known that addition of the mitotic poison colchicine to preovulatory mice leads to abnormal meiotic divisions which frequently produce nuclear-free eggs. Moreover, very recent work suggests that colchicine in-vitro acts similarly. When it is added to unfertilized eggs which have been surgically removed from a living mouse, healthy enucleated eggs are produced.

And furthermore it looks like the nuclear insertion process might not be anywhere as tricky as first thought. This change of opinion is the result of the development of very simple methods for fusing two cells to yield a single cell containing the genetic compounds of

both donor cells. Though the existence of rare examples of cell fusion was first clearly demonstrated in Paris by Barski in 1962, not until 1966 did Henry Harris and John Watkins working in the Pathology Department of Oxford University develop a routine method for easily fusing almost any two desired cells. Their contribution was the introduction of ultraviolet light-killed Sendai virus (a close relative of the common flu viruses). In some way not yet understood, adsorption of large numbers of Sendai particles so modifies cell surfaces that when two so-treated cells touch each other, portions of the opposing cell surfaces effectively dissolve, thereby creating one much larger cell containing two nuclei. Subsequently these nuclei often coalesce yielding a single nucleus containing all the chromosomes present in both original nuclei.

During the past 3 years Christopher Graham, also at Oxford, has been using Sendai virus to fuse mouse eggs with diploid adult mouse cells. The resulting cells still retain the essential features of an egg because even the relatively small mouse eggs are much larger than most diploid adult cells. While the fused eggs can divide several times, they so far have not yet developed into blastocysts, the stage necessary for successful implantation into the mouse uterus. Conceivably this limitation results from the need to remove the zona pellucida (a normal protective covering) for the Sendai virus fusing trick. Conditions must thus be found either to fuse eggs which retain the zona pellucida or which permit unprotected denuded eggs to develop normally to blastocyst. A reasonable guess is that Graham will succeed, if not this year, most likely within this coming decade. The clonal mammal then will no longer be science fiction.

A likely consequence will be initiation of similar experiments with a variety of other mammals; first, with easily obtainable laboratory varieties of hamsters, rats, and rabbits, and soon afterward with economically important domestic animals like cattle, sheep, and pigs. Though introduction of such methods into animal husbandry might seem at first like economic madness, many veterinarians may suspect otherwise, knowing well the very large prices currently paid for prize animals. Moreover, such research would certainly liven up many agricultural schools, since some of their faculty would jump at something more exciting than the now very routine breeding programs inspired by Mendelian genetics. So we must expect that, unless somehow strongly discouraged, veterinarians throughout the world someday will attempt the cloning of uniquely valuable domestic animals. One can certainly imagine wealthy race horse owners wondering whether with a better jockey their prize 3-year-old would have been unbeatable. While Nijinsky eventually lost his last two races, would a clonal derivative have always won?

At first consideration, it would seem likely that cloning of many domestic species would have to occur before serious thought would be given to the development of clinical procedures which would make human cloning more than a theoretical possibility. This way of thinking presupposes that the primary purpose for such methodological development need be cloning itself. If this in fact were the objective, the variety of normal and legal objections that would be bound to crop up most certainly would effectively prevent the legal granting of the medical facilities needed for extensive in vitro experiments with human eggs.

If, however, the stated objective is to probe the human reproductive process so that better contraceptive methods can be obtained, the reaction of the general public will be much harder to predict. Though many people will look with horror at any test-tube work with human eggs, others will breathe more easily that something is being done to prevent the world from being crushed by overpopulation. Until several years ago, this latter group was numerically relatively small and without favor in virtually any political circle. Today, however, taboos which would have seemed unbreakable just a decade ago are rapidly being overturned, witness the recent action of the U.S. Congress in overwhelmingly passing legislation that would promote family planning. Even more significant was the action of New York State in making abortions the right of any women who so desire them.

The prognosis thus seem virtually inevitable that for one reason or another the number of people studying all aspects of human embryogenesis will greatly increase. Not only will the amount of classical observational analysis increase, but even more important, direct experimentation with human eggs most likely will soon be the main preoccupation of a number of intelligent, highly qualified biologists.

Already there exists one such individual, R. G. Edwards, an English reproductive biologist now working in the Physiology Department of Cambridge University. Originally trained as an embryologist and with some 10-years experience in growing mouse embryos in vitro, he focuses his attention on the test-tube growth of human eggs. His original source of material was immature eggs (oocytes) obtained from ovarian tissue that had been surgically excised for reasons completely incidental to his work. From this tissue, Edwards removed the eggs from their surrounding follicles. As such, they were not yet capable of fertilization since most eggs within human ovaries are present in the dictyotene, a stage just at the beginning of the two meiotic divisions which generate haploid eggs. But by placing dictyotene phase eggs in a culture medium similar to that previously worked out for the in-vitro maturation of mouse eggs, the remaining

steps of meiosis occur and some thirty-six hours later bring the eggs to metaphase II, the stage where normal ovulation occurs. Then when human sperm were added, fertilization occurs yielding a diploid nucleus subsequently capable of dividing several times. However, no fertilized in-vitro matured egg has ever yet developed up to the blastocyst stage. Some factor not yet understood must go wrong during the in-vitro meiosis.

To circumvent this difficulty, Edwards, together with his clinical colleague, P. C. Steptoe of Oldham General Hospital, have devised a simple surgical method for the removal of healthy human eggs after they have completed much of meiosis, but before the ovulation step which releases free eggs from their follicles into the oviduct. Called laparoscopy, it is a relatively minor operation which, while requiring general anesthesia, generally only needs a 24-hour hospital stay. Prior to the operation, a regimen of hormone (gonadotrophins) treatment is given to induce follicle maturation and egg development through the early stages of meiosis. Laparoscopy is then performed, some 4 hours before ovulation would occur normally. The ovaries so exposed usually contain highly enlarged follicles with thinning walls, through which the desired oocytes can be carefully removed. Their procedures have now reached the state where they can obtain healthy eggs from over half the follicles examined.

Such preovulating oocytes are very suitable for subsequent embryological investigations. Fertilization rapidly ensues after human sperm addition, and in contrast to those eggs which had undergone meiotic divisions in-vitro, these in-vivo matured eggs generally begin normal cleavage divisions. Already many embryos have developed to the eight-cell stage while a few have become blastocysts, the stage where successful implantation into a human uterus should not be too difficult to achieve. In fact, Edwards and Steptoe hope to accomplish implantation and subsequent growth into a normal baby within this coming year.

The question naturally arises why should any women willingly submit to such operations. There is clearly some danger involved every time Steptoe operates. Nonetheless, he and Edwards believe that the risks involved are more than counterbalanced by the fact that their research may develop methods which make their patients able to bear children. All their patients, though having normal menstrual cycles, are infertile conceivably because many have blocked oviducts which prevent passage of their eggs into the uterus. If so, in-vitro growth of their eggs up to the blastocyst stage may circumvent their infertility, thereby allowing normal childbirth. Moreover, since the sex of a blastocyst is easily determined by chromosomal analysis, such women would have the possibility of deciding whether to give birth to a boy or a girl.

Clearly, if Edwards and Steptoe succeed, their success will be followed up in many other places. The number of such infertile women, while small on a relative percentage basis, is likely to be large on an absolute basis. Conceivably within the United States there could be 100,000 or so women who would like a similar chance to have their own babies. At the same time we must anticipate strong if not hysterical, reactions from many quarters. The certainty that the ready availability of this medical technique will open up the possibility of hiring out unrelated women to carry a given baby to term is bound to outrage many people. For there is absolutely no reason why the blastocyst need be implanted in the same woman from which the pre-ovulatory eggs were obtained. So many women with anatomical complications which prohibit successful childbearing, would be strongly tempted to find a suitable surrogate. And it is easy to imagine that many women who just don't want the discomforts of pregnancy would also seek this very different form of motherhood.

Some very hard decisions may soon be upon us. For it is not obvious that the vague potential of abhorrent misuse should weigh more strongly that the unhappiness which thousands of married couples feel when they are unable to have their own children. Different societies are likely to view the matter differently and it would be surprising if all come to the same conclusion. We must, therefore, assume that techniques for the in vitro manipulation of human eggs are likely to be general medical practice, capable of routine performance in many major nations within some 10 to 20 years.

The situation would then be ripe for extensive efforts, either legal or illegal, at human cloning. No reason, of course, dictates that such experiments need occur. Most of the medical people capable of such experimentation would probably totally stay clear of any step which in any way looked like its real purpose was to clone. But it would be shortsighted to believe everyone will instinctively recoil from such purposes. Some people may very sincerely believe the world desperately needs many copies of the really exceptional people if we are to fight our way out of the ever-increasing computer-mediated complexity that makes our individual brains so frequently inadequate.

Moreover, given the widespread development of the safe clinical procedures for handling human eggs, cloning experiments would not be prohibitively expensive. They need not be restricted to the super powers—medium sized, if not minor countries, all now possess the resources needed for eventual success. There furthermore need not exist the coercion of a totalitarian state to provide the surrogate mothers. There already are such widespread divergences as to the sacredness of the act of human reproduction that the boring meaninglessness of the lives of many women would be sufficient course for their willingness to participate in such experimentation, be it legal or

illegal. Thus, if the matter proceeds in its current nondirected fashion, a human being—born of clonal reproduction—most likely will appear on the earth within the next 20 to 50 years, and conceivably even sooner, if some nation actively promotes the venture.

The reaction of most people to the arrival of this asexually produced child I suspect will be one of despair. The nature of the bond between parents and their children, not to mention everyone's values about their individual uniqueness, could be changed beyond recognition, if such children became of common occurrence. Already many people, particularly those with strong religious backgrounds, believe we should *now* de-emphasize all those forms of research which could lead to circumvention of the normal sexual reproductive processes. If this step were taken, experiments on cell fusion might no longer be supported by federal funds or tax-exempt organizations. Prohibition of such research would most certainly put off the day when diploid nuclei can satisfactorily be inserted into enucleated human eggs. Even more effective would be to take steps quickly to make illegal, or to reaffirm the illegality of, any experimental work with human embryos.

Neither of these prohibitions, however, is likely to take place. In the first place, the cell fusion technique now offers one of the best avenues for understanding the genetic basis of cancer. Today all over the world, cancer cells are being fused with normal cells to pinpoint those specific chromosomes responsible for given forms of cancer. In addition, fusion techniques are the basis of many genetic efforts to unravel the biochemistry of diseases like cystic fibrosis or multiple sclerosis. Any attempts now to stop such work using the argument that cloning represents a greater threat than a disease like cancer is likely to be considered irresponsible by virtually anyone able to understand the matter.

Though more people would initially go along with a prohibition of work on human embryos, many may have a change of heart when they ponder the mess which the population explosion poses. The current projections are so horrendous that responsible people are likely to consider the need for more basic embryological facts much more relevant to our self-interest than the not-very-immediate threat of a few clonal men existing some decades ahead. And the potentially militant lobby of infertile couples who see test tube conception as their only route to the joys of raising children of their own making would carry even more weight. So, scientists like Edwards are likely to get a go-ahead signal even if, almost perversely, the immediate consequences of their "population money" supported research will be the production of even more babies.

Complicating any possible effort at effective legislative guidance is the multiplicity of places where work like Edwards' could occur, thereby making most unlikely the possibility that such manipulations

would have the same legal (or illegal) status throughout the world. We must assume that if Edwards and Steptoe produce a really workable method for restoring fertility, large numbers of women will search out those places where it is legal (or possible), just as now they search out places where abortions can be easily obtained.

Thus, all nations formulating policies to handle the implication of in vitro human embryo experimentation must realize that the problem is essentially an international one. Even if one or more countries stop such research, their action could effectively be neutralized by the response of a neighboring country. This most disconcerting impotence even holds for the United States. If our congressional representatives, upon learning where the matter now stands, decided they wanted none of it and passed very strict laws against human embryo experimentation, their action would not set back seriously the current scientific and medical momentum which brings us close to the possibility of surrogate mothers, if not human clonal reproduction. This is because the relevant experiments are not being done in the United States, but largely in England. This is partly a matter of chance, but also a consequence of the advanced state of English cell biology. In certain areas it is far more adventurous and imaginative than its American counterpart. Now there is no American university with the strength in experimental embryology that Oxford possesses.

We must not assume, however, that today the important decisions lie only before the British Government. Very soon we must anticipate that a number of biologists and clinicians of other countries, sensing the potential excitement, will move into this area. So even if the current English effort were stifled, similar experimentation could soon begin elsewhere. Thus it appears to me most desirable that as many people as possible be informed about the new ways of human reproduction and their potential consequences, both good and bad.

This is a matter far too important to be left solely in the hands of the scientific and medical communities. The belief that surrogate mothers and clonal babies are inevitable because science always moves forward, an attitude expressed to me recently by a scientific colleague, represents a form of laissez-faire nonsense dismally reminiscent of the creed that American business if left to itself will solve everybodys' problems. Just as the success of a corporate body in making money need not set the human condition ahead, neither does every scientific advance automatically make our lives more "meaningful." No doubt the person whose experimental skill eventually could bring forth a clonal baby would be given wide notoriety. But the child who could grow up knowing that the world wants another Picasso would view his creator in a different light.

I would thus hope that over the next decade wide-reaching discussion occurs, at the informal as well as formal legislative level about

the manyfold problems which are bound to arise if test tube conception becomes a common occurrence. On some matters a sufficient international consciousness might be apparent to make possible some forms of international agreements before the cat is totally out of the bag. A blanket declaration of the worldwide illegality of human cloning might be one result of a serious effort to ask the world which direction it wishes to move.

Admittedly, the vast effort, needed for even the most limited international arrangement, will deter those who believe the matter now is of such marginal importance that in effect it might be a red herring designed to take our minds off our callous attitudes toward war, poverty, and racial prejudice. But if we do not think about the matter now, the possibility of our having a free choice will one day suddenly be gone.

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SUMMARY VIEWS AND COMMENTS

HERMAN POLLACK [1]

I find that my mind is a bit awhirl with the richness of the material which has been placed on our platter during the past three days.

Many wise—indeed some profound—observations have been offered on the status, the problems, and the prospects of international science. Anticipating my assignment this morning, I have been tentatively testing over the past few days several observations, hopefully original, which I might contribute to this dialog. With each passing hour, however, one by one my tentative themes have been preempted by other speakers. Collectively, you are a hard act to follow.

Moreover, I call your attention to the fact that four simultaneous panels on two successive days have created an information explosion with which, I submit, not even UNISIST or the latest IBM computer could contend. Were I not such a trusting soul, I might suspect that the arrangements of the last two days were ingeniously designed to make it impossible for the man who was to deliver summary remarks to fulfill his function.

However, I have read the papers as they became available and have received reports on sessions which I could not attend. And it does seem to me that there are several themes which recur in the comments we have heard.

One is the importance of insulating science from the imperatives of parochial politics. Several speakers referred to the low specific gravity in political terms of the problems dealt with by science and of the ability this gave science to cross lines of political difference.

Repeated attention was called to the thesis that the habit of cooperation, which is fostered by scientific relationships, in itself is of high value and a justification for the relationship. Numerous speakers called attention to the importance of more effective use of science and technology in support of the developmental aspirations of the poorer countries of the world.

Hardly a speaker failed to emphasize the importance of the free movement of scientific information among the countries of the world. And finally, almost without exception every participant in this seminar alluded to the necessity of employing science and technology more effectively in the achievement of the great social aims of this age.

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And, of course, we had an especially stimulating discussion on the environmental issue. Dr. Watson's presentation suggests to me that in the area he described there is a pressure generated by scientific progress to define a social aim.

I must be forgiven for not refraining from pointing out that with the possible exception of the last point each of these was incorporated in the keynote remarks of the Secretary of State. They have, of course, been enlarged and illuminated by succeeding speakers. It should now be apparent that these are all basic points which will have to be considered as the international community proceeds in the development of international science policy.

I should like to comment now on several points which arose in the dialog of the three-day panel. First, I would like to say that anyone who occupies my position cannot help but be impressed by the verity of the theses that international scientific relationships must be insulated from transitory political considerations and they have possibly unique capabilities of transcending political differences.

Certainly we in the Department of State subscribe wholeheartedly to these points. At the same time, I want to underline the importance of a point that Mr. Daddario, among others, touched on in his remarks. This is that the absence of political agreement, frequently occasioned by national ambitions or concerns regarding sovereignty, is an effective barrier to many necessary endeavors in science and technology which can be accomplished only by international cooperation. The undeniable success of CERN is, so to speak, the exception that proves my rule.

Another way of stating my point is that we may sometimes unbalance our perspective by overly emphasizing the necessity to free science from political considerations. I concur entirely that this is a valid concern. At the same time I submit that we have not emphasized nearly enough the importance of obtaining the political agreement which will be the necessary precedent to the multilateral undertaking of major scientific and especially technological ventures such as those that are foreseeable, for example, in the use of outer space and in the management of international environmental problems.

Achieving international political agreement is, of course, an immensely complicated task—certainly as difficult as any scientific challenge ever confronted by a scientist or scholar. Political agreement is much too important a task to be left to the diplomats. It will require the active participation of every skill and activity represented in this room, and more besides.

If I understand what we were doing in the early part of this morning's session, we were indeed tussling with the problem of political agreement, but it is not clear to me whether the discussion, especially of international waters, advanced or set back the cause.

A second comment which I would like to make relates to the goals to which international science policy is directed.

The overall objective of governments in fostering international cooperation in science and technology is to advance their national interests and to strengthen their international relationships. I have stated that rather bluntly, but this is a fact of life which should be taken as an operating premise by all who are interested in enlarging governmental support for international scientific cooperation.

Inherent in all such cooperation is the desire to extend, improve, or expedite the acquisition and diffusion of knowledge. Such cooperation, furthermore, is often activated or motivated by humanitarian, political, or economic considerations.

From this cooperation, each government expects to obtain particular benefits, direct or indirect. In some cases, these will be tangible and of an economic nature. Others will be less tangible, such as improvement of health, safety, the quality of life, and of course the advancement of science, the thread which binds the entire enterprise.

These goals and benefits are not unilateral, of course. Cooperation would not be possible or meaningful if the goals sought were not mutually compatible and the benefits derived did not flow to each nation involved.

As my third comment, I suggest that the scientists sometimes hide their light under a bushel. There have been many at this meeting, and elsewhere, who have alluded to the lack of public and political support for science. It seems to me that if there were wider understanding, that international scientific and technological cooperation frequently does pay off in tangible ways, often economic, that public and political support might be more forthcoming.

Secretary Rogers in his remarks spoke of the tangible benefits which the United States has realized from its participation in cooperative international programs in science and technology. He referred to the saving of lives through better hurricane warning systems and to foreign contributions to the U.S. lunar program.

These examples were drawn from a study the Department of State has under way of the tangible benefits of international cooperation in science and technology. This study, which must be described as preliminary, has identified literally hundreds of specific examples of specific tangible benefits to our citizens generated through cooperative international programs in science and technology. Our partners in this cooperation can of course make the same claim.

Our listing embraces many instances of *direct economic benefit:* through sharing with other nations the costs of essential research; through the incorporation into key U.S. research programs of instrumentation, techniques, and essential data generaged in programs supported by other nations; and through opportunities for U.S.

scientists to utilize unique research facilities—ranging from oceanographic research vessels to special collections of biological materials—created by and financed by other nations.

Our study has identified many important examples of *indirect economic benefits* accruing from international cooperation in science and technology. Thus, there are the new markets for U.S.-manufactured scientific instruments which result from international cooperation in research, and the adoption by U.S. producers of economically important new technologies developed abroad and brought to our attention as a result of cooperative programs.

Another important form of indirect economic benefit is the ability to avoid unproductive, and expensive, directions for our research efforts or to "leapfrog" in our research planning on the basis of results coming to us through international cooperation. It is difficult—if not impossible—to assign specific dollar figures to these indirect economic benefits, but in many instances they are clearly substantial.

No one can assign objective dollar values to the results of basic research or the development of new technologies which relate to improving the health of our citizens, or to public safety. These are, nonetheless, tangible benefits, and our international cooperation in science and technology has yielded dozens of important gains in these areas.

I do not wish to overstate the importance of tangible benefits resulting from international cooperation in science and technology nor assert that they alone are an adequate justification for scientific cooperation. On occasion, however, that is precisely the case.

I do assert, however, that we are not taking sufficient credit for the practical, tangible, and economic benefits that frequently result from scientific cooperation and from the uses to which we put the knowledge that is derived from such cooperation.

Were we to do so, perhaps the hard-headed budgeteers and appropriators of money might be more receptive to requests for funds for scientific endeavors.

Policy for science and technology is, in a very real sense, policy for the future of all mankind. These annual meetings of the House Committee on Science and Astronautics with the Panel on Science and Technology, incorporating each year the contributions of outstanding scientists and administrators from many parts of the world, have played a unique role in the public discussions of this vital subject. They constitute one of the best examples I know of successful international scientific cooperation.

Congressman Miller and his committee deserve a great deal of credit for showing the way. We in the Department of State—and I think I speak for all of us in the room—are grateful for the opportunity to participate.